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Psychological Bulletin

SOME PSYCHOLOGICAL PROBLEMS OF ECONOMICS

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I. Introduction

It is the practice these days to genuflect before the ideal of interdisciplinary cooperation, as a prerequisite to progress toward the distant goal of a single, basic, integrated social science. Most discussions of the interrelationships of the social sciences emphasize the need for cooperative research, and philanthropic foundations have abundant funds to support projects that bring together the different social sciences. A great impetus to such cooperation, moreover, comes from the practitioners of applied social science; faced with the concrete problems of government, business, education, diplomacy, the social science technician quickly finds that his recommendations are no more reliable than the weakest links in the chains of reasoning supporting them. Usually trained in only one, or at most two, social sciences, the social scientist on the firing line is forced to look for help from others whose training will supplement his own.

Yet, for all the good intentions held and expressed, far too little cooperative social science is practiced. Partly, this is an inevitable result of the great accumulation of social science knowledge, making narrow specialization the obvious route to expertness and advancement. Partly, it comes from a tradition of cloistered and solitary scholarship, and from the personality structure of many who find such scholarship congenial. And partly, it comes from interdepartmental jealousies in universities, business enterprises, and government. But there remain many social scientists with an interest in and a competence for cooperative, interdisciplinary research. The present paper has been prepared to suggest some of the many possibilities for fruitful collaboration between economists and psychologists in the study of economic behavior. It considers primarily the needs of economics for psychological supplementation, recognizing at the same time that cooperative inves-

[1] On leave from Dun & Bradstreet, Inc.

tigation of the problems posed would add greatly also to the development of psychology, particularly social psychology.

The available literature, particularly in economics, contains many discussions of the theoretical relationship of psychology and economics (**37, 39, 40, 43, 49, 51, 55, 56, 57, 58, 81, 84, 86, 97, 102, 109, 112, 113, 120, 125, 129, 130, 131, 133, 136, 137, 138, 142, 148, 149, 167a, 174, 175, 190, 195, 199, 200, 202, 204, 212, 212a, 215, 215a, 237, 238, 239, 260, 269, 273, 284, 299**). There are also a number of empirical studies of the psychology of consumer spending and saving, and of labor relations. Few studies of business behavior exist, however, where there is any real merging of psychological and economic concepts and techniques, resulting in new data and insights significant for both disciplines (although see **117, 118, 119, 180, 246**). Moreover, some studies of the economic psychology of consumers (**36a, 36b, 36c, 36d, 119a, 119b, 119c, 148a, 148b, 297, 301**) may suggest principles and techniques applicable to entrepreneurial psychology. This paper will first consider briefly the general relationship of psychology to economics, and will then suggest some of the particular problems of entrepreneurial psychology that, being central to economic theory, merit a high priority in any cooperative psycho-economic research program.

II. Economics as the Study of Behavior in Production and Exchange

Among the economists who have systematically discussed the relationship of psychology to economic theory, and the ways in which psychological concepts and data might contribute to a fuller understanding of economic behavior, there are represented two opposed points of view about the task of economics itself, as a distinctive discipline. One view would define the task of economics as the study of a *particular area of subject matter*, viz., man's activities in production or exchange situations, and would consider of interest to economic theory *all* aspects, or all *measurable* aspects, of behavior in that area. The second view is that economic theory should concern itself only with the *abstract economic aspect* of social behavior, i.e., with the formal implications of the rational pursuit of money.[2]

Economics as an Area of Human Behavior

Up to the early nineteen-thirties, economists of the classical tradition, as well as most of the critics of classical orthodoxy, accepted the *subject-matter* rather than the *aspect* view of the task of economics.

[2] This distinction is brought out very clearly by Parsons (200), to whom parts of the following discussion are heavily indebted.

Their aim was to develop an economic theory that would make understandable as much as possible of the behavior involved in production and exchange. Whether they started from a priori assumptions about human nature, or from extensive and detailed statistics, both orthodox and unorthodox economists sought to establish general principles that would characterize economic behavior in the "real" world.

Classical Orthodoxy

When these "subject-matter" economists develop economic theory along orthodox lines, they start with five a priori psychological assumptions and, making extensive use of the *caeteris paribus* condition, seek increasing "elegance" in developing the formal implications of these assumptions under this condition (and under additional conditions as they seem pertinent) (112). The psychological assumptions made (although not always explicitly) by orthodox classical theorists are:

1. That production and exchange are predominantly (if not exclusively) motivated by the attempt to *maximize money gains*, usually in the short run.
2. That economic behavior is predominantly (if not exclusively) *rational* and based on reasonably complete *knowledge* of the market and reasonably perfect *expectation* of the results of alternative choices. (Otherwise, assumption [1] would be meaningless.)
3. That additional units of consumption (and hence of income) bring progressively less satisfaction (Gossen's Law of diminishing marginal utility).
4. That total wants inevitably exceed the means of satisfying them (the principle of scarcity).
5. That satisfactions are to some extent substitutable for each other.

Utilizing these assumptions (sometimes called "a priori facts" [112]), and requiring that "all other things remain equal," orthodox economists derive general "laws" or "tendencies" (such as the "law" that price will move towards that point where supply and demand are equated, or the "tendency" of the economy to approach general equilibrium). They justify their method by claiming that these "laws" or "tendencies" do, in fact, adequately describe ("explain") by far the largest part of economic (and particularly business) activities.

Critics of Classical Orthodoxy

Orthodox economists do not differ from most unorthodox economists in what they are trying to do (i.e., generalize about a subject matter, a particular area of human behavior). The difference comes in terms of judgments of the adequacy of the method chosen to achieve their common goal. Unorthodox critics of the classical method insist on the importance of additional or alternative psychological assumptions, deny the broad applicability of *caeteribus paribus*, or turn to empirical

data (instead of "a priori facts") to provide the bases for their generalizations. Unorthodox economists in this "subject matter" tradition thus seek to build up an economic theory not limited to explaining such economic behavior as is dominated by rational profit-maximizing, but able to explain in addition the great part of concrete economic behavior that is conditioned by social institutions (above all by the forms of business organization), by habits and customs, by population and the laws of its growth, by scientific knowledge and its resultant technology, as well as the great part that is motivated by non-economic considerations, such as satisfaction in doing a job well, in achieving prestige or status, in dealing honestly, in obtaining and exercising power over others, in creating, or in acting in harmony with the "spirit," the value-system, of the particular epoch. When the task of economic theory is defined thus broadly, economic theory tends to become a general theory of social behavior, applied to the particular problems of business and its associated activities. So oriented, economic research contributes to the development of a unified social science whose principles are applicable to any area of behavior.

Adequacy of Psychological Assumptions of Classical Economic Theory

Orthodox classical economists, being satisfied with the adequacy of the conventional psychological assumptions of economics, usually express the best of intentions towards psychology, but do not take it seriously enough to allow it to influence their thinking and research. They consider self-evident the fact that most production and exchange behavior is adequately described by the generalizations of classical economics (modified where necessary to take into account monopolistic or collectivistic organization of production) which result from the logical development of their basic psychological assumptions. Why, then, complicate the structure of economic theory by introducing concepts of behavior motivated otherwise than by profit-maximization, or influenced by non-rational (i.e., impulsive or habitual) factors? Furthermore, to the extent that the economist admits the significance in economic behavior of non-rational or non-profit-motivated factors, he must either measure these factors and introduce them into his theory, or fail in his task of developing valid generalizations about theoretically expected reactions to price and income changes (because he is then faced with multiple equilibria and with indifference curves that cannot be drawn in terms of measurable units.)[3]

[3] See, for example, Higgins (108) and Boulding (27).

If economic psychology is considered unimportant by many economists and, even when admitted to be important, is found to be a theoretical obstacle, it is small wonder that psychology gets such summary treatment in most modern texts on economic theory.

And yet, the classical theorist can be quickly led astray by his reliance on the single motive of profit-maximization; for example, consider the classical discussion of interest. Consistent with assumption [1] above, an explicit and more precise psychological assumption underlying the theoretical framework of the famous "stationary state" was that saving (viewed primarily as "abstinence") was largely motivated by the money paid as interest on savings. It followed therefrom that the rate of saving would decline sharply if the interest rate fell below a certain level; hence that "over-saving" could not occur for long, as it would drive the interest rate down until current consumption became more attractive than the command over a slightly larger future consumption.

Much more realistic and, in fact, absolutely essential to an understanding of economic trends was the Keynesian recognition that the rate of saving is *little* influenced by the rate of interest, that saving has a complex and predominantly *non-pecuniary* motivation, and that the act of investment, which implies the loss of the liquidity of accumulated savings, is governed by still another and quite separate group of predominantly non-pecuniary motives (**123**). In this case, at least, the bland assumption of the orthodox classical economists that most economic behavior is dominated by rational profit-seeking leads to palpably false conclusions. This falsity was demonstrated by economists themselves (which may have made the demonstration easier to accept), but it is quite clear that a thorough psychological investigation of the saving behavior of individuals would have brought the classical assumption into question much earlier. It is equally clear that saving behavior should now be studied by psychological techniques, examining into the "arm-chair" hypotheses that Keynes termed "propensity to consume" and "liquidity preference," and developing techniques for quantifying the data on these motives that can be obtained by observation and by interviews with the individuals directly concerned.[4]

There are many other areas where common-sense observation shows the predictive inadequacy of an economic theory that takes into account

[4] The interviews with businessmen carried out by the Oxford economists in the late nineteen-thirties (**7, 103, 168, 224**), and the surveys of consumers liquid asset holdings being carried out for the Federal Reserve Board by the Survey Research Center of the University of Michigan (**119a, 119b, 119c, 148a, 148b, 297, 301**), suggest the kind of research needed to develop a satisfactory theory of interest.

no motives other than rational self-interest. Workers may turn down employment opportunities at better pay and with better living conditions, preferring to stay in the community and occupation to which they are accustomed, even if that means living on the "dole" in a "depressed area." Businessmen may forego the large profits of smart, aggressive, competitive success for the sake of a quiet, sheltered, habit-drugged business existence so long as their security and position in the industry are not threatened. (See also **151.**) Explanation and prediction of such behavior demands a subtler and more complex approach than classical economic "laws" make possible.

It is important to remember, when considering the "economic laws" of the classical theorist, that these are not "laws" in the sense of generalized descriptions of empirical data. If they were, the confusion about the real nature of the interest rate could never have arisen. Economic "laws" are simply *rational type cases.* They are *norms* for economic behavior, indicating what is to be expected *if* economic behavior is rational and seeks to maximize pecuniary gain.[5] (In addition to the psychological assumptions mentioned above, assumptions are usually made about the degree of mobility of labor and capital in adjusting itself to demand, the degree of competition among buyers and among sellers, divisibility of the commodity, the possibilities of substitution, and the absence of sudden and drastic changes in the economy.) Economic "laws" are *felt* to be empirical, by most economists, because common experience and common sense testify that many people in many situations do in fact seek pecuniary gain and calculate rationally what is to their best advantage. But empirical laws based on scientifically collected data might be quite different from these norms (as in the case of saving behavior), and would certainly be phrased differently. Empirical "laws" would state *how much*, *where*, and *when*, not simply *what.* They would be stated as propositions that could be tested, and retested, against existing or newly collected data (**29, 39, 41, 98, 112, 142, 195, 216, 218, 235, 257, 275, 286**); and they would take into account the great differences between individuals who felt able to influence their economic environment (such as major business or labor leaders) and those who felt they had little such control (small business men, consumers, employees) (Cf. **278a**). One of the first tasks of economic psychology should be to study individuals systematically in their varying roles as consumers, savers, investors, workers, managers, lenders, inventors,

[5] Hutchison (**112**) goes so far as to conclude that the traditional propositions of deductive economics are devoid of any empirical content. See (**113, 120, 137**) for a discussion of this position.

landlords, builders, etc., to find out, among other things, how important the rational pursuit of money really is in their economic actions, how its importance changes from role to role and from time to time, and how, in particular, its importance is modified when counterbalanced (or reinforced) by other powerful motives, such as patriotism, need for security, desire for prestige, personal affection, etc.

So much for the orthodox classical economists. Satisfied that their "laws" explain most significant economic behavior, especially in the long run, and that economic data obtained by conventional statistical methods are all they need in order to develop and verify those laws, they smile in neighborly fashion at psychology but are careful not to welcome such a disturbing element into their well-ordered household.[6]

To some extent, we have been flaying a dead horse. The critics of classical orthodoxy have been many, and little contemporary economic theory has escaped the influence of their criticisms. However, before considering the unorthodox movements that, in attempting to generalize about the same "subject-matter," have sought to supplement the classical assumptions with additional psychological elements, brief mention will be made of the logically tenable but extreme position vis-a-vis psychology that is taken by the few economists who subscribe to the *aspect* view of the task of economics.

III. Economics as the Study of "Economizing"

It is only in recent years that the attacks of institutionalists on orthodox economic theory have been met by the highly sophisticated argument that economic theory should not be expected to explain *all* behavior in economic situations, but should limit itself to an analysis of the economic *aspect* of human behavior, leading to the development of abstract economic principles that describe the behavior that *would* result from rational attempts to maximize pecuniary return *if* all other

[6] Weisskopf (**278**) has made an entertaining analysis of economic theorizing, and especially its choice of certain assumptions and its abstraction from the real world of conflicting interests and anxiety-inducing change, in terms of psychoanalytic defense mechanisms: "isolation" (theoretical abstraction), "projection" (utilitarianism, where man's economic wants "naturally" lead to the "good" of society), "repression" (equilibrium assumed, and antithetical elements repressed), and "compensation" (assumptions of omniscience and perfect rationality made as compensation [fantasy gratification] for some of our main failures). His thesis (**278a**) that classical economic theory is a projection into nature and into the universe of the money-motivated, unintegrated, and anonymous role forced upon the individual by the machine process suggests that a more rounded economic theory is not likely to be widely accepted except in a society in which the individual has a greater opportunity to express himself *as a totality*.

factors (both motives and conditions) were held constant (**52, 97, 112, 133, 149, 174, 199, 200, 215, 215a, 238, 239**).[7] Those taking this position would leave to other abstract bodies of theory (such as psychological theory, sociological theory, political theory, etc.) the task of explaining the rest of the social behavior observed in economic situations. The proponents of this view recognize, of course, that the solution of concrete economic problems and the development of specific economic policies require that economists obtain the cooperation of other social scientists. They point out, however, that scientific progress is possible only through the process of abstraction, and that the time for interdisciplinary cooperation is not at the stage of formulation and theoretical development of general principles, when it merely clouds the issues, but rather at the stage of their application to concrete problems and policies, when it is of transcendent importance (even though its importance is not always recognized.)

For these abstractionists, the relationship of psychology to economic theory is very simply stated, as follows. However helpful psychology may be when one is attempting to solve concrete economic problems and frame economic policies, economic theory as such need have no truck with psychology. True, abstract economic theory cannot be considered significant unless one accepts the validity of its basic psychological assumptions (above, p. 291); but common experience can testify to the essential validity of these assumptions (without denying that other psychological factors affect economic action). Furthermore, these psychological assumptions are held to be *constants* so far as economic theory is concerned. Hence psychological measurement of and generalizations about their variations are necessarily excluded.

Orthodox classical economists, subscribing to the subject-matter view of the task of economics, and abstractionists, holding to the aspect view, come out at about the same place with respect to psychology, though for different reasons. The former consider psychology unimportant or a nuisance; the latter consider it irrelevant. Both groups run a double risk: (1) major areas of economic behavior (such as saving or working) may in fact be so dominated by motives other than the rational pursuit of money that theorists must either reach faulty conclusions

[7] A related view holds that economic "laws" simply describe the *conditions necessary* for the stability and autonomy of the market process as a whole. If such laws are inoperative, market equilibrium is impossible; and, in fact, deviation from these conditions may be used to appraise the efficiency of the economic system in getting the most out of its resources. Explanation and prediction of exchange transactions and market processes require the supplementation of these "laws" by the empirical data and principles of social psychology (**150, 151**).

concerning them or must declare them outside the range of their legitimate interest; and (2) conclusions reached on the basis solely of abstract economic analysis may be mistaken for final conclusions, without taking back into account the psychological (and other) elements that were put to one side for the purpose of simplifying the problem. This "fallacy of misplaced concreteness" (ignoring the incompleteness of principles derived by abstraction) is a fallacy that frequently tempts theoretical economists when they consider concrete problems of policy.

IV. Additional Psychological Factors Introduced into Economic Theory

Economic theory has not, of course, succeeded in remaining "pure." Counterbalancing the tendencies to simplification and abstraction noted above have been strong pressures to recognize the importance of psychological, sociological, political, and legal factors. Considering only the psychological elements, we find that Adam Smith (**236**) recognized that man's "moral sentiments" and regard for public opinion play an important part in limiting the behavior of the economic individualist, and Ricardo spoke of the "habits and customs of the people" as being significant determinants of much economic behavior (**214**, p. 55). It was left to Malthus (**160**), however, to become the first economic theorist really to incorporate psychological elements into equilibrium theory. According to Malthus, economic tendencies could not be analyzed by the assumption of rational profit-seeking alone. One had to take also into account man's procreative urge, leading to a continual pressure of population against the ceiling on consumption set by a niggardly Mother Nature (in the form of the principle of Diminishing Returns).

The Malthusian Theory of Population was for a long time an integral psychological part of classical economic theory. It was eliminated only when empirical data—statistical data—proved it to be invalid, at least for much of the Western World. Its importance as an economic factor in other areas should not be ignored on this account, however. There is still need for detailed studies of the psychological factors influencing the birth rate and the size of families in many parts of the world today, if economic theory is to have general significance.

Psychological Hedonism

Another psychological element became attached to classical economics for a while, without really influencing its theoretical superstructure. Impelled by a felt need to find a psychological foundation for eco-

nomics, to explain the nature of consumer demand by tying it to something more fundamental, the economists of the nineteenth century, such as Jevons (**114**) and Pantaleoni (**194**), accepted the postulate of a general all-pervading psychological motive, the pursuit of pleasure (and avoidance of pain), and "explained" the demand for a certain commodity by its utility in giving pleasure.[8] "Psychological hedonism" played a part in economic theory for a long time, and turns up occasionally even today.

"Psychological hedonism" did not add anything to classical economic theory, however, for it simply begged the question it set out to answer. The only way one could tell what gave pleasure (of the general sort postulated by the psychological hedonists) was to find out what was demanded (**273**). Furthermore, as was pointed out later, economic theory can be built up quite satisfactorily without knowing anything about what underlies peoples' wants, so long as one knows and can somehow measure those wants (as by knowing how much money will be paid to satisfy them, or which products will be preferred over others) (**43, 53, 106, 107, 130, 138, 186, 221, 222a**, esp. Pt. I; **225, 246, 254, 255, 275, 281**). This does not imply that the area of consumer demand presents no psychological problems to be investigated. On the contrary, we know that the attitudes and motives underlying demand are multiform, unevenly distributed in the population, imperfectly correlated with the ownership of the means for making them effective, variable over time and under the barrage of modern advertising techniques, and intercorrelated in varying patterns; and investigating these problems with adequate social science techniques yields much which is directly relevant to the understanding of economic behavior. But this implies measurement and the collection of new data; "psychological hedonism" was a sterile concept because it did not lead to independent measurements useful in the prediction and control of economic behavior.

The psychological hedonists did not concern themselves with demand only. They wanted also to explain differences in wage rates, and they were not blind to the fact that man's willingness to do certain jobs is greater than his willingness to do others. Taking a rather dim view of work in general, they explained relative willingness to work in terms of the "pain" associated with particular occupations. They did not analyze the matter further.

More acute observers, however, recognized that there are positive as well as negative values associated with work. Marshall analyzed a number of activities that were satisfying in themselves and not as means to ulterior ends, viz., energy, initiative, enterprise, rationality, industry, frugality, and honorable dealing. T. N. Carver introduced into his eco-

[8] Acceptance of this general principle stimulated the development of elaborate classifications of wants and motives (**22, 22a, 56, 104**, esp. Ch. II; **114**).

nomic theory the motive of devotion to work for its own sake, his "work-bench philosophy." Veblen spoke of an instinct, or "bent" of workmanship. "Psychic income" is a familiar modern term for the satisfaction derived from the nature of one's work. All these concepts could supplement classical theory, without necessarily denying the domination of profit-maximization in much economic behavior. They all suggested fruitful areas of psychological research, although it was some years before psychological techniques were equal to the task of exploring them.

Instincts in Economics

Despite the sterility of the concept of psychological hedonism, the desire to base economics on a psychological foundation continued strong, and economists somewhat slavishly followed the next fashion in psychology. The chapter on instinct in James' *Principles of Psychology* (published in 1890) may be said to have started the conversion of economists from complete dependence on the rationalistic hedonism of the utilitarians. Following the publication in 1908 of McDougall's famous "Social Psychology," the instinct theory was adopted by many economists, impressed to find the vagaries of human behavior so easily and clearly "explained." They promptly started writing economics in these new psychological terms (**54, 55, 56, 61, 80, 110, 161, 172, 176, 196, 197, 237, 247, 249, 250, 251, 268**).

Despite its popularity, the instinct hypothesis did not represent much of an advance in social science. Experimental investigation and systematic observation (primarily by the behaviorists) quickly showed that humans did not inherit rigid patterns of response to social situations. Every writer had his own list of instincts, comprising groups of activities in which he saw some unifying element; but there was no way to identify behavior motivated by the "instinct of pugnacity," for example, except to agree that certain behavior appeared pugnacious. The terms "pugnacious instinct," "instinct of accumulation," etc., could not mean anything operationally for prediction and control until measurements of the instincts could be carried out independently of the economic behavior they were invoked to explain; and psychologists were unable to provide such independent measurements.

On two scores, however, the instinct fad was a step forward. The variety of human motivation was stressed (in contrast to the generalized pleasure-pain formula of the psychological hedonists), opening the way to research into particular attitudes and motives; and the analogy to animal behavior drew attention to the need for objective investigation, in contrast to the introspective, arm-chair approach.

In another direction also, the instinct theorists deeply influenced economic thinking. Their work cast serious doubt upon the rationality of economic behavior. Psychoanalytical writers were also emphasizing

the importance of non-rational, subconscious drives, and the unreliability of "rationalizations;" while behaviorists were, on quite different grounds, beginning to argue that social scientists should disregard all introspective data. Economists consequently found it difficult to continue to think of conscious, rational profit-maximization as the dominant guide to economic action. This had two unfortunate results. On the one hand, interest in classical economic theory diminished and its development was retarded. On the other hand, those who continued theorizing in the classical tradition cut themselves off from contemporary currents of psychological thought, and their distaste for and distrust of psychology in economics endures to this day.

Veblen occupied a unique position, in this respect. He was an instinctivist, postulating three ultimate instincts pertinent to economic action (parental bent, bent of workmanship, idle curiosity); but his instincts were explicit, conscious "ends of action." Pecuniary reward was not, however, one of these ultimate goals, and he denied that the rational pursuit of money was an organizing principle in economic society. Moreover, he and Marx were among the few economists willing to recognize that goods and services were very widely obtained *outside* the market, in ways other than by exchange (i.e., by gift or by seizure) (see **84**, pp. 11–15; **250**; **272**, ch. 6). Unlike the critics of classical economics mentioned above, Veblen wanted not merely to broaden existing theory; he wanted to replace it with a theory he found more satisfying (**6**, **52**, **84**, **263**, **264**, **265**, **266**, **267**, **268**, **269**).

Farthest away from the classical tradition was the economic theory of the German Historical School, of which Marx and Sombart were the most prominent members. According to them, there was no possibility of a universally valid economic science, because the ultimate goals of economic action, the motives underlying the organization of production, were impermanent, were not characteristic of man in general, and were applicable only to a single epoch in man's history. (See also **149**, **277**, **278a**.) Thus, the study of the psychological motives underlying economic behavior was not simply a method of broadening one's economic theory; it was essential to an understanding of that theory, and was the only way one could tell whether that theory had any validity for the present day and age.[9]

[9] Classical economists themselves were not as thoroughly committed to the concept of a universally valid economic science as sometimes represented. J. S. Mill pointed out that a person is not likely to be a good economist who is nothing else: ". . . . social phenomena acting and reacting on one another, they cannot be rightly understood apart. . . . . the material and industrial phenomena of society are . . . . susceptible of useful generalizations, but . . . . *these generalizations must necessarily be relative to a given form of civilization and a given stage of social advancement.*" (**171**, p. 81) (Italics mine.) There are also suggestions in Marshall (**165**) that ultimate wants are not fixed, that they may become "adjusted to activities," changing in nature as the organization or technology of society changes (see Parsons, **198**). Clark (**40**) has an interesting discussion of the possibility and economic implications of the malleability of wants.

Institutionalism and Later Unorthodox Developments

Since Veblen's day, most efforts to add to the parameters of economic theory have been grouped together under the broad and rather amorphous concept of "institutionalism," meaning in general (1) dissatisfaction with the adequacy of classical theory, (2) the suggestion of additional principles governing economic behavior, and (3) an emphasis on the importance of objective facts, versus deduction from logical but not necessarily empirical principles (**6, 37, 49, 51, 52, 56, 57, 84, 89b, 93, 109, 129, 131, 167a, 173, 174, 175, 190, 191, 195, 200, 201, 203, 208, 209, 212, 212a, 212b, 228, 237**, 250, **260, 261, 282, 283, 284**). Institutionalism has done much more to disrupt theory, however, than to improve it. Without formulating or making possible the formulation of a new theoretical structure, institutionalists attack the adequacy of the old, thus becoming in effect not merely anti-classical, but actually anti-theoretical.

The failure of institutionalists to erect a new theoretical structure may be ascribed to something more fundamental than an antitheoretical bias. To build, one must have tools, and the most essential tools (for this purpose) are measuring instruments. For the classical theoretical structure, money provides the measuring instrument. Demand (though not all demand) can be measured in money; costs (though not all costs) can be measured in money; and reactions to economic changes can be predicted by assuming the dominance of money-profit-maximization. As Marshall emphasized (**165**, Bk I, Ch. II, and Appendix D, par. 2), the theoretical structure of economics could not have been elaborated without the measuring instrument provided by money, for the progress of science depends on measurement, and other measuring instruments have been wanting.

What the institutionalists lacked, and what prevented their development of a new theoretical structure, was measuring instruments for the determinants of economic behavior that are not expressible in monetary terms, that is, for most motives, attitudes, habits, expectations, etc. Fortunately, this lack is today being supplied—at least in part—by the new techniques of social science. The application of these new techniques will be discussed later in this paper. Here, it is enough to point out that we are on the verge of a new flowering of economic theory, a theory that will take into account (in measurable terms) many of the psychological elements whose exclusion from economic theory has so long been protested by the critics of classical orthodoxy.

Related to the institutionalist tradition are two recent developments in economic theory, viz., (1) the emphasis on psychological factors in dynamic process analysis, and (2) the controversy over the adequacy of marginalism in the analysis of the firm. It is interesting that, with the shift in interest in macro-economic theory from emphasis on static equilibrium analysis to an emphasis on dynamic process analysis, there has come also an increased recognition among economists of the impor-

tance of psychological elements in economic behavior. For example, of the four factors stated by Keynes (**123**) to be adequate to explain the level of employment, three are psychological, namely "the propensity to consume," "liquidity preference," and the subjectively expected "marginal efficiency of capital." In most dynamic process analysis, "expectations" play a key role.[9a] Econometricians usually leave room for psychological factors to be introduced into their mathematical models, if and when psychologists can measure such factors. Most other economists concerned with public policy likewise stress the significance of psychological factors in determining the level of economic activity (e. g., **183**, esp. pp. 49–51; **188**, esp. p. 27; **234**, esp. pp. 43–62).

In micro-economics, also, the importance of supplementing pecuniary marginalism with other elements has gained increasing support. Following the publication of data obtained by the unorthodox but sensible (although often technically unsophisticated) method of interviewing individual businessmen (**7, 8, 24, 67, 92, 103, 117, 144, 158, 168, 223, 224, 294**), an active controversy has raged concerning the adequacy of the marginalist approach to the theory of the firm (**48, 53, 69, 89, 111, 144, 145, 146, 146a, 156, 157, 192, 193, 211, 213a, 245**). In summing up the evidence, R. A. Gordon (**89**, pp. 287–8) concludes that traditional marginalism has been pretty well worked out, that further empirical research limited to the tools of marginalist theory is likely to be sterile, and that significant advances in economics are now to be expected only if empirical workers are supplied with new and more useful analytical tools. (Also see **26**, pp. 801–802; **97, 142, 167a, 216, 218, 272, 286.**)

As we survey the history of economic thought, it is apparent that economists, even in their theories, have never been satisfied with the concept of the purely "economic man." They have had to consider many of man's behavior traits. As Mitchell points out, "Keynes' concepts of the 'propensity to consume' and 'liquidity preference', so confidently invoked today, are as patently psychological as Adam Smith's 'propensity to truck, barter and exchange', or Bentham's 'felicific calculus', or Malthus' 'instinct of procreation', or Bagehot's 'cake of custom', or the Austrians' 'marginal utility', or Edgeworth's 'indifference curves', or Veblen's 'cultural incidence of the machine process', or Schumpeter's distinction between 'routineers' and 'innovators', or Pigou's epidemics of 'over-optimism' and 'over-pessimism', or Freud's 'complexes' hiding in the 'subconscious'. The 'economic man' was a psychological caricature, deliberately drawn to facilitate speculation, but condemned in advance to be a sketch of how creatures

[9a] Analysis of expectations is just as subject to highly refined deduction from unsubstantiated psychological assumptions as is any other sector of economic theorizing (**139a, 167a, 231, 232**).

very differently endowed than human beings would behave" (**178**, pp. 16–17).

The goal of any science is generalization, prediction, and—eventually—the ability to control. For any area of economic behavior, the goal of economic science should be to formulate valid generalizations concerning the conditions under which certain actions take place, to predict that certain conditions will be followed by certain actions, and to prescribe the steps to be taken to bring about the particular actions desired. If people could in fact be depended upon to act "*as if*" profit-maximization were their goal, and rational balancing of alternatives their method, there would be no need to complicate economics with more psychology. If further psychological assumptions promise to improve prediction and control, however, it would be absurd to exclude psychology from economics on the grounds either that it seems a priori to be unimportant, or that its inclusion would destroy the distinctiveness of the economic discipline. Most economists undoubtedly consider that they have a legitimate interest, as economists, in *all* of the factors influencing the organization and processes of production and exchange.[9b] The crux of the matter is whether or not psychological data, principles, and research techniques promise to increase substantially our capacity to understand, predict and control economic behavior, and this question can only be answered after the fact, not before it.

V. The Psychological Approach to the Study of Economic Behavior

In general, the psychological approach in studying behavior differs in important ways from the economic or the statistical approach. First, data are sought concerning the behavior of *individuals*. Aggregates, averages, and indexes may be utilized, but only after the *individual* variations underlying them have been investigated. Second, data are sought by going *directly* to the individual, not relying on statistical records through which behavior can only be studied after the fact and at second- or third-hand. Third, *all* kinds of habits, attitudes, and motivations are considered, not simply profit-seeking. And finally, consideration is given to the ways in which attitudes and motivations *change*, and the techniques that might be useful in changing them. Investigators other than psychologists may approach economic problems in these ways, but these are the usual characteristics of the psychologist's approach. (Cf. **34**, **102**, **119**, **126**, **148**, **212**, **212a**, **212b**, **237**).

[9b] Even those economists whose interests lie primarily in the development of formal theory along classical orthodox lines recognize the importance of empirical research in cooperation with other social scientists (**64**; **215**, ch. IV; also see **39**, **58**, **81**, **109**, **149**, **167a**, **272**, **273**, **284**, **285**, **299**).

Emphasis on Individual Differences

Psychologists do not have a monopoly on the individual approach. The whole section of economic theory that has to do with the behavior of the firm is essentially a study of hypothetical (and sometimes real) individuals (see above, p. 302). Moreover, the realization of the actual (and growing) extent of imperfect competition and the resulting possibility of administered prices has led to increasing emphasis on understanding the behavior of the individual oligopolist, whether price leader or follower.

Quite as important as understanding the individual entrepreneur, of course, is understanding other strategic individuals in the economic arena—the labor leader, the policy officers of great financial institutions, government officials, leaders of opinion and fashion in the community—whose economic decisions have an impact far out of proportion to their number. Today, we live in what is to a large extent an *administered* society, rather than a society whose character results from the interplay of countless decisions, among which none has a disproportionate effect (**86**, **105**, **185**). An understanding of the bases of administrative decisions is now a necessary supplement to, and perhaps even outweighs in importance, an understanding of the statistical tendencies of mass opinion and action.

Obtaining Data at First Hand

In studying individual behavior, the economist has generally been content to use as data the records collected by others—records of new orders, posted prices, plant construction, labor turnover, etc. These data may be unreliable or unsuited to his needs (because collected with other needs in mind). They are often poorly dated and nearly always too superficial to reveal what really took place at the time the essential economic decisions were made (**34**, pp. 6–8; **102**, pp. 87–89). The use of psychological research techniques (such as the case study,[10] sample surveys utilizing depth interviews, content analysis, or experimental social psychology) could be expected to illuminate the behavior *underlying* these surface records.

Each individual who might be the subject of psycho-economic study

[10] A particularly promising technique in studying business behavior is the case-study. If mature investigators, well-trained in economics, social psychology, and interviewing techniques, could sit in regularly on policy discussions of industrial concerns, and could interview intensively the significant policy-makers over extended periods of time, they should be able to develop a body of case material showing *how* policy decisions are arrived at, what considerations are given what weight, etc., that would add greatly to our present knowledge and understanding. This technique is now being utilized in the Brookings Institution study of industrial concentration and in the University of Illinois study of business expectations. Also see **206**, **213a**, esp. pp. 305–308.

typically plays more than one economic role.[11] He is certainly a consumer, probably also an employee or entrepreneur, probably also a saver, possibly also an investor in securities. Other possibly overlapping roles are those of the government policy-making official and the civil servant, the financier, the employed manager, the engineer or other professional staff man, the voter, etc. While the same individual may and usually does play more than one of these roles concurrently, he usually acts in any given economic situation as though he were playing only one role. For the time being he is dominated by a conception of himself as a particular bundle of habits, attitudes, objectives, etc., and he may and often does act in one role in ways that are inconsistent with his behavior in other roles (e.g., an entrepreneur or an employee may seek a higher direct return from an enterprise even though the result may be that as a consumer he has to pay higher prices). There are very great differences in the groups of motives dominating the different economic roles man plays, and there are great differences in the motives dominating the different kinds of economic action that occur within any one role. For meaningful psycho-economic research, therefore, it is necessary to break down the variety of economic behavior into sub-classes having some homogeneity—both in terms of economic significance and in terms of superficial psychological similarity. In section VI, there is suggested an a priori breakdown of classes of action taken by individuals playing the role of ***entrepreneur***. This breakdown indicates the great variety of separate psycho-economic investigations needed for each role played by economic actors.

Limited Area Theories

Studies that are each limited to a particular class of economic action may be expected to result in the supplementing of general economic theory by a series of particular theories of economic behavior, each comprising generalizations or "laws" that are empirical (rather than "type cases") and each appropriate to a given area of economic activity—these areas perhaps being defined both in economic terms and in geographic terms. For example, farm ownership behavior may be quite different in Pennsylvania, where the descendants of Dutch settlers abhor being in debt for anything, from what it is in the South, where it is traditional to borrow even against current crops. Similarly, the tradition of the textile industry, where one gets ahead by competitive cost

[11] Economic "role" is not meant to refer simply to one's *economic interests* (as a consumer, entrepreneur, etc.), but to the much more significant concept of the *entire* behavior pattern appropriate to a particular role. This "picture in the mind" of what is *fitting*, which takes into account *all* one's attitudes and motives, provides a much more complete and reliable basis for predicting economic behavior than does the limited concept of rational economic interest.

and price cutting, contrasts sharply with that of the metal industries, where much more time and effort has gone into attempts to work out *non*-competitive arrangements (cf. **26, 101**).

As a separate group of psycho-economic generalizations (a "theory") is developed for each area of economic activity, it will become possible (1) to predict reactions to purely economic changes and controls; (2) to develop a range of possible supplementary psychological controls; and (3) to predict the likely success of these psychological controls. The prediction and control techniques developed may be expected to be tailored differently for each class of economic actions found to be psychologically distinct, if the greatest effectiveness is to be attained.

Variety and Malleability of Motivations Affecting Economic Behavior

A major difference between the approach to economic research dictated by classical (including most current) economic theory and the approach utilized by the psychologist lies in the variety of determinants of economic behavior considered significant, and in the constancy attributed to them. The economist's theoretical interests are limited to a few psychological determinants (those that impinge, or are believed to impinge, on profit-maximization) and he tends to look upon these as relatively unchanging. His theory has been unable to shield him from the facts, however, and the distinction between the economic approach and the psychological approach is consequently fuzzy. The empirical literature of economics is studded with insights into the non-rational and non-pecuniary determinants of economic behavior, and with gropings for psychological principles that can supplement the principle of rational self-interest. These tentatives are usually isolated, unsystematized, and undeveloped in terms of their psychological origin, function and characteristics. Nevertheless, they demonstrate how strongly economists have felt the need to widen and deepen the psychological foundation of their theoretical and predictive structure. This economic psychologizing constitutes a standing invitation and a challenge to psycho-economic investigation, for its significance to a fuller understanding of the economy is quite apparent from the importance of the problems which it is called upon to help solve.

Two fields of psychology are consequently of particular significance to economics, viz., *learning* (including communication) and *motivation.*

The principles of habit-formation, the extent of habitual behavior in economic situations, the significance of the "Gestalt," the characteristics and incidence of insightful learning, and the importance of suggestion, imitation and sympathy, have many implications for the understanding, prediction and control of economic behavior (**118**). Advertising and propaganda are based upon them. Much economic behavior can only be understood in the light of the advertising and propaganda (and sometimes the education) that have impinged upon particular

economic groups. And the achievement of control of economic behavior (whether by private groups or by government) often rests more upon adequate use of techniques of communication than upon economic or legislative actions. Human beings are malleable, and economists who take man's economic psychology as a given, without caring how he got that way or what possibility there is of changing him, are contenting themselves with a much less effective role than they might play.

In the field of motivation, also, there are psychological concepts and principles of particular value to economists. There is special significance in the modern understanding that motives are not fixed and invariant, or universally present (except in very general terms); and that wants are all cultural, at least in the forms in which they are significant.[12] Most needs, wants, desires that play an important part in economic behavior are oriented toward fairly specific goals. They have, however, become attached to those goals during the individual's prior experience, and, within limits, they can be detached from one goal and attached to another. Within broad classes, there is thus the possibility of substitution of one goal for another. But this malleability and substitutability do not mean that the existence of presently effective motives cannot be demonstrated, nor that the incidence and relative strength of these motives cannot be measured. To a degree, these motives are measured indirectly by market behavior, choice of vocation, volume of saving, etc.; but direct investigation of particular individuals can be a much more fruitful method of identifying and evaluating the strength of these motives, in such a way that economic behavior can better be understood, predicted and controlled. No investigation of particular economic behavior is complete unless the investigator discovers:

1. What persons are active and influential in making significant economic decisions? How can these policy and decision makers be identified, and what techniques of investigating them promise to be most fruitful? What kinds of interaction are involved in the process of decision-making?

2. What attitudes, beliefs, expectations, habits, knowledge, and motives are important, and how influential are they in each decision-making situation? How many (and which) people have these attitudes, knowledge, etc., and how strongly? Which attitudes are reliably correlated with *action?*

3. What is the origin and developmental history of these attitudes, etc.? What are their sources in the culture (general and specific culture areas)? How are they transmitted (through what educational channels, what media of communication, what kinds of opinion leaders, etc.)?

4. How malleable are existing attitudes, how receptive are economic actors to new knowledge, etc.? How does receptivity to new knowledge, rumor, sug-

[12] Many economists, particularly among the institutionalists, have stressed this concept, but all have been content with descriptive accounts (**4**; **5**, Ch. I; **21**, **40**, **47**, Pt. VI; **56**, **125**, **136**, **165**, Bk. III, Ch. II; **190**, Part I; **198**, **202**, **204**, **210**, **264**).

gestion, etc., vary among individuals, and what factors are related to such variations? What psychological techniques are available to influence attitudes and through them each class of economic actions? How effective can these techniques be made?

Research Needed Even with the Simplifying Assumption of the "Economic Man"

Although most economists (like other social scientists) recognize the inadequacy of the single motive of "rational self-interest" as a determinant of economic decisions in real life, there is a wide area where psycho-economic research is needed even within the limits of this concept of motivation. Every economic decision is made in a particular economic *situation*, comprising (1) all the elements of the institutional framework (laws, political system, customs, etc.), (2) all the economic data (levels of wages and prices, production costs and sales volume, level of profits and interest rates, strength and efficiency of competitors, etc.) *known* to the actor and believed by him to be *pertinent* to his decisions, and (3) all of the actor's *expectations* about the future behavior of these institutional and economic factors.

Knowledge of pertinent facts not a safe assumption. There is first the question of what facts and factors are considered by the actor to be *pertinent* to his decisions. There has been a great deal of arm-chair speculating about the kinds of facts and factors that *should* be recognized to be pertinent, if the economic actor were analytical and far-seeing. (Appendix A outlines many of these institutional and economic factors.) There is very little that is really known, however, about the range even of the strictly economic considerations that influence economic behavior, and even less that is known about their relative importance as influences. Furthermore, this is a field where belief is as important, and probably more widespread, than knowledge. The gyrations of the stock-market are *believed* by many businessmen to be significant for them, not because of their economic effects (on the interest rate, on the ease of financing, or on their own personal finances) but because of their presumed psychological effects on the expectations of other business men. Most businessmen *believe* that certain economic statistics are particularly significant as indicators of changes in business conditions, but the actual significance of most such indicators is obscure and considered by business-cycle students to be meaningful only in the context of the whole economic situation. Economists have often stressed the importance of the interest rate, and assumed that it would be an important element in business calculations; yet few business men apparently consider it very significant for their own operations (**7, 67, 103, 168, 224**).

Again, economists speculate about the theoretical effect on economic behavior of the absence of complete knowledge of pertinent data, but there is practically nothing known about the actual *extent* of knowl-

edge among various economic actors today. How much do entrepreneurs, consumers, employees *know* about the facts of their own economic situations (e.g., **26**, **81**, pp. 61–63; **153**)? What are the sources of their information, and how reliable are these sources? How can their knowledge be increased? What changes in behavior ordinarily result from more knowledge of particular kinds? Here is a whole world that needs exploration.[13]

Expectations a key field for psycho-economic research. Even more important for economic decisions than how much is actually known about current economic conditions, however, are *expectations* about future economic trends, about competitor reactions, about governmental taxation and controls, etc. The *concept* of expectations is central in modern economic theory (**99**, **100**, **106**, **112**, **118**, **128**, **139a**, **180**, **226**, **231**, **232**, **256**, **271**). The *facts* about expectations are almost unknown and constitute a major field demanding psychological investigation. Because of the importance of expectations for all the fields of economic behavior, some of the questions about expectations that should receive early attention in a psycho-economic research program are outlined here:

1. *Whose* beliefs and expectations are important? How identify the economic opinion leaders? Flow of opinion from whom to whom, and through what channels (**17**, **17a**)? Are industries whose turning points "lead" the business cycle headed by business opinion leaders? Note that expectations of large companies and of "price-leaders" are much more important than those of small; sampling should stress those individuals who change *first*, and who have the greatest impact.

2. How do expectations develop (**117**, **226**)? What media containing economic information, business opinions, and forecasts reach what people (**205**)? What are taken how seriously? What factors give recipients confidence in the reliability of their informants? How important are wish-thinking and differences in susceptibility to rumors of different sorts? Can the effects of long-run susceptibility be separated from those of short-run predispositions resulting from temporary insecurity?

3. How easily do expectations change? Under what conditions is change facilitated? What psychological techniques may be effective in changing business expectations (**18**, **298c**).

4. How strongly are expectations held? Can strength of conviction (resulting in action?) be measured separately from persistence (unwillingness to consider contrary information)? Are expectations typically much more confidently held and more slowly changed when they refer to consumer behavior than when they refer to business (and particularly *investment*) behavior? Is certainty or uncertainty greater at the height of a boom?

[13] The fields of market research, commodity research and cost accounting have, of course, been very greatly developed in the last twenty years to meet this very need. Little is known about how widely these kinds of information are utilized, however, it is important, furthermore, to recognize the great significance of particular statistical methods in influencing business behavior. Different accounting assumptions, for example, may introduce important biases into what is "known" (**77**, **241**, **294**).

5. How variable are expectations at any one time? Can variance be used as a predictive factor?

6. Is there greater unanimity about "general business" than about one's own industry or concern? Are there several "climates of opinion" held by the same people? Which affects business behavior most importantly, and how (**117, 291, 300**)?

7. Methods of measuring expectations of business men (**117, 180, 253, 291, 292, 300**) and of farmers (**31, 72,** pp. 428–429; **296, 298b**).

8. Measures of validity of expectation data, including reinterviews to determine how subsequent action is related to expectations, and correlation of expectation series with business activity series (**33**). Is there a tendency for expectations to "justify" themselves (**18, 28,** p. 138)?

9. Nature and influence of individual concern's expected demand curve (**32, 139a, 213a, 231, 232, 244, 259,** esp. pp. 78–108; **271, 298b**).

10. Nature and influence of expectations about future course of wage rates, materials costs, selling prices, etc. (**20, 106, 117, 139a, 184, 287**).

11. Changes in business motives in the light of changing expectations (e.g., high profits sought during business expansion, security sought during contraction) (**1**).

12. Measurement of impact of particular events—such as changed government policies—on business "confidence." Importance of knowledge that particular monetary, fiscal or other governmental measures will be taken in stated circumstances (**163**).

13. Effects of changing business expectations on price and production decisions (**78, 117, 139a, 231, 232, 298b**).

Beyond the "Economic"

It is obvious, then, that, even accepting the simplifying assumption of a single economic motive of rational self-interest, there is plenty of need for psycho-economic research; but the field is far wider. Most economic behavior in real life also involves other motives and much behavior of an habitual character. From a psychological point of view, economic behavior is as complex as any other social behavior, and requires as comprehensive an approach—with only the limit that the behavior (decisions) studied should have generality and be of significance for the economy.

Within economics as a whole, the behavior of entrepreneurs (including their investing behavior) has received the greatest attention in economic theory, and at the same time has drawn the least on psychological concepts and techniques. Neither the behavior of workers nor the behavior of consumers has received nearly as much attention from theorists, and both have had much more attention from psychologists.[14] The outline below is limited to the behavior of entrepreneurs, and to the areas of entrepreneurial behavior that are particularly promising

[14] Theoretical studies of the consumption function are, of course, important and fairly numerous, and these are increasingly tied to empirical investigations—some of which draw heavily on the psychological approach.

for inter-disciplinary research. It is the purpose of this outline (1) to point out the areas of entrepreneurial decisions that have special significance for economic theory, (2) to indicate the variety of nonpecuniary motives (and some of the psychological complexity of the "basic" profit motive) to which economists themselves have found it necessary to appeal in developing generalizations about economic behavior, and (3) to aid prospective psycho-economic teams to find the literature (primarily economic) that will be helpful in setting up the theoretical economic questions that need empirical investigation by such teams. (These references are not exhaustive, however, nor do they constitute the source of all the psychological factors listed.) It is the hope that presentation of this outline will stimulate psycho-economic research that results in the quantification of the incidence, influence, origin and malleability of the factors listed.

VI. Critical Areas of Entrepreneurial Behavior Requiring Psycho-economic Investigation

Meaningful investigations of enterpreneurial behavior can only be designed if the major classes of entrepreneurs are distinguished and studied separately, thus holding constant some of the important variables related to economic behavior (112, esp. pp. 163–165). It is apparent that the effective psychological factors, and hence the likely decisions, are quite different for individual farmers from what they are for corporation farmers, or for small retail trade and service entrepreneurs, for large corporations engaged in retail and wholesale trade and services, for small or large manufacturing concerns, for small or large banking and financial establishments, for publicly regulated utilities, or for government owned and operated enterprises. The economic populations selected for study should, besides being categorized as above, be further sub-classified in such a way as to eliminate the effects of variations in the characteristics of economic activity and institutions outlined in Appendix A.

As entrepreneurs carry out their economic functions, the areas of decision most important for economic theory are: entering or leaving an industry, keeping one's capital more or less liquid, raising more capital in different ways and from different sources, introducing technological or organizational changes, formulating price and production policy, gaining market advantages through means other than price competition, selecting distribution channels and sources of supply, and determining wage levels. (Decisions concerning procedures and techniques of employment, sales promotion, industrial relations, and participation in political activity are less important for economic theory.) These major areas are treated separately in the following outline.

A-1. *Area of decision to be taken by entrepreneur: whether to enter an industry, continue in it, expand in it, or leave it.*

Decisions to establish an enterprise; to buy into a going enterprise; to increase investment in an enterprise, or the scale of its operations; to take on additional lines, produce additional products, offer additional services; to plough profits back into the business and thus expand investment; to maintain investment, refusing to expand in any way; to make inadequate allowances for depreciation and thus reduce investment; to drop some lines, products or services; to withdraw capital or reduce the scale of operations; to sell a portion of the enterprise; to sell or disperse the whole operation.

A-2. *"Non-economic" factors considered likely to influence decisions* (excluding economic data and expectations).

Extent of personal participation, as where the enterprise becomes one's major occupation or even a *way of life* (as in farming, small retailing, or the professions).

Importance of personal independence.

Prestige or social status of particular industry, or of ownership or managerial position *vs.* being an employee of someone else.

Mechanical, commercial or professional aptitudes and interests.

Scope for imagination and inventiveness.

Desire to participate in constructive enterprise—the feeling of *creating* something.

Desire to build or preserve a family institution and economic base.

Family or cultural traditions.

Readiness to take risks.

Urge to dominate.

Desire for security of capital or stability of income (rather than maximum total profit).

Attitude towards "rights" of stockholders, *vs.* managerial desire to build up concern.

Sense of responsibility to owners, employees, customers or community for maintaining and perhaps building up economic activity.

Readiness to take advice or get training on conduct of business, and on factors necessary for success (vocational counseling, small business institutes).

Adaptability of management, workers and organization to new design or new product; or their attachment to established line.

References: **8, 9, 15, 26, 27, 36, 46, 62, 65, 71, 73, 82, 85, 87, 89a, 100a, 114a, 115, 123, 127a, 146a, 155, 164, 222a** (esp. Pt. I), **227, 237, 248, 262, 266, 268, 279, 280, 293.**

B-1. *Area of decision to be taken by entrepreneur: what form to keep existing capital in.*

Cash, government notes, government bonds, other securities unrelated to conduct of business, securities providing some measure of control over related business operations, accounts receivable (including consumer credit), loans (commercial or personal), inventories, plant, equipment, real estate, patents, technical processes and other research results; training, welfare or morale of

labor and management; goodwill of consumers, distributors, suppliers, employees, or general public.

B-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations).

Felt need for "safety-margin" of quick assets.
"Liquidity preference."
Ability to judge credit-worthiness (e.g., "character" appraisals of applicants for credit).
Belief in ability to judge credit-worthiness.
Felt security of capital in each form.
Traditional or habitual financial practices in the industry or company.
Desire to control operations where capital invested.
Desire to reduce or prevent competition with existing interests.
Desire for flexibility provided by liquidity of assets.
References: **10, 45, 70, 73, 74, 75** (esp. p. 152), **85, 90, 95, 96, 99, 100, 100a, 106, 123, 127, 128, 146a, 164, 170, 274, 276, 298a.**

C-1. *Area of decision to be taken by entrepreneur: whether to raise additional capital.*

To strengthen current financial position; to strengthen control (as by owning rather than leasing property); or to enable expansion of operations.

C-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations).

Attitudes toward owned versus rented property.
Attitude towards being in debt.
Desire to control enterprise.
Ability to tolerate insecurity arising from shortage of liquid capital assets.
Desire for flexibility provided by liquidity of assets.
Attitude toward expansion of operations (see A, above).
Belief in importance of repaying debts, in "sacredness" of contract.
Attitude towards going through bankruptcy.
References: **7, 8, 67, 91** (esp. pp. 39–44), **96, 103, 124, 132, 134, 135, 139, 140, 141, 152, 164, 168, 222, 224, 227, 230, 234, 252, 258, 270, 274, 288, 289.**

D-1. *Area of decision to be taken by entrepreneur: how to raise needed capital.*

Loans from banks, RFC, or other lending institutions; issue of bonds or debentures; issue of preferred or common stock; borrowing from officers or other individuals; or holding back earnings for reinvestment.

D-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations)

Attitudes toward desire of creditors for some measure of control over policies and operations.
Attitude toward stockholders' "right" to control reinvestment of earnings.
Effect on prestige of borrowing at a high rate of interest.

Attitude toward open market treatment of securities, with their implications for prestige of company.

Insecurity generated by need to make regular payments (e.g. on loans).

Reference: **274.**

E-1. *Area of decision to be taken by entrepreneur: whether to change technology or organizational arrangements.*

New or improved tools or machinery, or strains of plants and animals (in farming); work flow and factory layout; activities or functions based on time and motion studies; supervisory responsibilities; customer participation (self-service or self-assembly); utilization of new types of professional or managerial advice; statistical quality control; operations analysis and market research; cost analysis and accounting methods; encouragement of research; suppression of inventions.

E-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations).

Receptivity to innovation on part of management or labor force.

Relations between management and labor force.

Anticipated competitive position.

Anticipated control over improved technique.

Stage in growth of an industry (new techniques easier to introduce during a period of expansion).

Anticipated effect on employment, wages, conditions of work.

Attitude towards discarding usable but obsolete equipment.

Attitude towards going into debt.

References: **19, 25,** ch. 9; **44, 66, 79** (pp. 70–80), **121** (n. 4, p. 170), **159, 162, 169, 181, 182, 242** (esp. pp. 59–66), **295.**

F-1. *Area of decision to be taken by entrepreneur: price and production policies in the short-run* (i.e., involving no change in investment).

Expanding or restricting volume of production; altering prices directly or indirectly (i.e., by changing quality, services provided, terms of sale, liberal credit, etc.); changing selling costs, including relative expenditures for advertising, public relations programs, etc.

F-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations).

Beliefs about what industries are really competitive with own.

Expected reaction of sales to price change. (Expected elasticity of demand.)

Relative importance of "lucrativity, safety or asymmetry" of expected net receipts.

Extent of understanding of marginalist concepts and thinking in marginalist terms.

Belief in a "fair" price or a "fair" profit.

Belief in high-volume-low-profit policy vs. high-profit-restricted-volume policy.

Group attitudes of producers toward "price-cutting."

Identification with, and importance of prestige and status among, producers group.

Community attitudes towards charging "fair" prices.

Belief in importance of goodwill of consumers, distributors, employees or public at large.

Identification with, and importance of prestige in, community.

Identification of own interest with that of customers.

Expectations of retaliation by customers when conditions change or even in other relationships (ostracism, voting of governmental controls, even physical violence).

Feeling of obligation to ownership interests.

Governmental controls, and attitudes of group, community and individual towards compliance therewith.

Desire for security, liquidity, or flexibility of capital or for maintenance intact of going organization.

Desire to demonstrate personal independence or power.

Belief in short-run and long-run effectiveness of advertising and public relations programs.

Willingness to seek and use advice on management policy.

Willingness to adapt policies and management to competitive pressures.

Belief in and willingness to use particular accounting practices (such as average vs. marginal unit costs, proper differentiation of capital expenditures and operating costs, different depreciation policies, etc.).

Closeness of personal relationship between decision-maker and purchaser.

References: **6, 7, 8, 14, 14a, 23, 26, 29, 31, 45, 53, 76, 77, 78, 83, 89, 89a, 92, 94, 98, 102, 103, 108, 111, 116, 117, 123, 127a, 143, 144, 145, 146, 146a, 158, 166, 168, 177, 179, 187, 189, 207** (esp. chs. VI & VII), **211, 220, 222a** (esp. Pt. I), **223, 224, 226, 229, 237, 240, 243, 279, 290.**

G-1. *Area of decision to be taken by entrepreneur: how to maintain or strengthen market position by means other than price competition.*

Advertising; differentiating products; making production and marketing agreements; licensing agreements; basing point systems; mergers; trade associations; obtaining legal limitation on entry of new concerns, on level of production, on increased investment, on sale in particular areas, or on price-cutting ("fair-trade" laws), etc.; cooperative marketing; cooperative purchasing; cooperative production; patents; or use of non-economic pressures, such as friendship, persuasion, ostracism, publicity, etc.

G-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations).

Feeling that competitive results are too harsh in particular industry.

Feeling of inability to control own production quickly (e.g., weather, in farming).

Belief in a "fair price," or a "fair profit."

Belief in "fair share" of market, or right to a "fair" living. (Note anti-chainstore legislation, control of number and kind of lawyers, doctors, barbers, etc.)

Community support of local concerns.

Understanding of exchange economy, especially in field of international trade.

Feeling of ability to set own prices, vs. compulsion to follow "price-leader" or "the market."

Belief in "free competition" as a basic and desirable element in the economy and in own industry.

Loyalty to industry association or cooperative.

Climate of business opinion (business tendency to seek controls in bad times and throw them off in good.)

Fear of court interpretation of "intention" in merging.

Belief in need for and desirability of governmental controls in own and other industries.

Belief in strength of, and attitude towards, economic behavior of other economic groups.

References: **2, 11, 12, 16, 30, 35, 38, 50, 68, 83, 167, 189, 217, 227, 259, 293.**

H-1. *Area of decision to be taken by entrepreneur: what customers to sell to.*

Directly to users or consumers; to retailers (a few large or many smaller); through own retailers; to wholesalers; to jobbers; or through food stores, variety stores, drug stores, etc.

H-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations).

Knowledge of current distribution practices and results.

Desire to promote own brand.

Fear of being at mercy of one or a few large buyers.

Belief that "fair" price will be paid.

Preference for or avoidance of sharp bargaining.

Need to retain goodwill of existing customers.

Loyalty to friends, neighbors, and other habitual customers.

Estimate of credit-worthiness of customers.

Prestige gained or lost by sale to particular users, retailers, etc.

Attitudes of compliance with governmental regulations (rationing, priorities, set-asides).

Attitude towards price-maintenance by dealers.

Attitude towards engaging in price competition.

I-1. *Area of decision to be taken by entrepreneur: choice of sources of supply and of capital.*

From what particular source of supply to buy raw materials, supplies, services, equipment, fuel, goods to resell, etc.; where to raise money for additional capital.

I-2. *Psychological factors likely to influence decision* (excluding economic data and expectations).

Habitual or traditional channels of supply.

Community pressures or individual attitudes for or against (a) particular social, political, religious, or racial groups, or (b) types of economic organization.

Reciprocity.
Loyalty to organization (cooperative, grange, etc.).
Friendship.
Desire to have dependable source of supply.
Dislike of dependence on too few sources.
Desire to control source of supply.
Fear of control by source of supply or by creditor.

J-1. *Area of decision to be taken by entrepreneur: whether and how to change wage levels or composition of labor force.*

Altering wage rates (without changing kind of rate); changing from hourly rate to piece rate, or vice versa; establishment or modification of bonus or incentive systems; changing wage structure (as by job analysis and classification); changing other components of compensation (vacations, pensions, etc.); hiring more employees or reducing labor force; changing proportion of particular classes of labor (e.g., special skills or unskilled labor replaced by new machinery); or varying hours of labor (over-time or part-time work, or change in regulation work-day) instead of numbers of employees.

J-2. *Psychological factors likely to influence decisions* (excluding economic data and expectations).

Community estimate of a "fair" wage, and community pressures to comply therewith (either by raising wages or by resisting labor's demands).

Industry estimates of a "fair" wage and industry pressures to comply therewith.

Personal relationships between owner or manager and labor force (as on farm, in small business establishment, family assistants in retail store, etc.).

Belief in "right" to freedom of managerial decision in wage questions.

Feeling of freedom of managerial decision (limited by governmental controls, labor strength and attitudes, community and industry pressures, etc.).

Traditional wage levels or differentials (e.g., low pay in banking).

Appraisal of importance of company to the community (services, payroll, etc.)

Feeling of responsibility for social impact of wage level or labor force decisions.

Understanding of needs and motives of employees, of unions, and of union leaders.

Belief in possibility of cooperative modification of wage rates or wage structure.

Knowledge of attitudes of labor force towards particular changes.

Belief in importance of keeping a going organization (especially key men) through bad times.

Expected reaction of employees to wage action or inaction.

References: **59**, **60**, **63**, **76**, **101**, **122**, **144**, **145**, **147**, **213** (esp. pp. 285–287), **213a**, **219**.

APPENDIX A: *Stratifying the Economic Sample*

In planning an empirical investigation of economic behavior, it is important to identify and hold constant as many as possible of the independent variables that are known or presumed to be able to influence the behavior studied. On in-

vestigation, it may turn out that some of them do not in fact influence this behavior, for the entrepreneur's knowledge of the situation is never complete, and much of what he knows he may ignore (**26, 81, 153**). Until it is known what elements really are important in given situations, empirical generalizations must be built up separately for highly homogenous and fully described strata (**14, 26, 101**).

The most important independent variables of a *general economic* nature that must be held constant or otherwise "partialed out" are: the levels of prices, the levels of wages, the volume of sales, the interest rate, the availability of particular commodities or services, the level (and kind) of taxes, the level of profits, business activity (in general, in the local community, in a given industry, and for a given company), the productivity of land, machinery, and labor, and the existence of war or other crises.

The most important *institutional* factors that must be taken into account in stratifying data are:

1. *Kinds of commodities or services produced or distributed.* The outstanding variables here, so far as decision-making is concerned are: (a) durable deferrable-purchase commodities (like automobiles) *vs.* non-durable (especially perishable) non-deferable-purchase commodities (such as cigarettes); (b) differentiated (usually brand-named) commodities (like soap) *vs.* standard commodities (like wheat); (c) consumers goods (like clothing) *vs.* producers goods (like industrial belting); (d) goods representing large purchases in a family or business budget (such as a machine) *vs.* goods whose purchase has little effect on a budget (like stationery) and which, in the case of consumers, is subject to "impulse" buying; (e) luxury commodities or services, especially if bought for prestige purposes, *vs.* essentials; (f) relative proportion of labor cost to total cost; (g) degree of individual craftsmanship.

2. *Size of company*, especially the number of people employed therein. Bureaucracy flourishes in industry as well as in government, and faces many of the same problems of diffusion of the decision-making process, red tape, passing the buck in risky decisions, inadequate communication between different elements, internal rivalries, jurisdictional disputes, etc.

3. *Spatial dispersion of company units*, and degree of responsibility allowed each unit. This factor may intensify the problems of a large company mentioned in (2) or may result in separate decision-making in many small, relatively independent units. The location of some units or operations outside the national boundaries of the country where the home office is located introduces, of course, new factors into the decision-making of local units.

4. *Stage of development of the industry.* A company in a "growth industry" (like television today) gives weight to quite different considerations from those considered important by a mature or declining industry (like black gunpowder manufacturing).

5. *Length of cycle of production.* In much farming several months, and in some plantation production (rubber, cocoa, bananas) several years, are needed before a decision to increase or reduce the scale of production can be made effective without disproportionate losses. Likewise, manufacture of locomotives or ships may involve a long cycle of production that markedly influences the psychology of decision-makers in that industry.

6. *Kind of market served.* Selling to many potential customers involves quite different considerations and tactics from selling to only a few. Where a substantial part of production is destined for export, additional considerations enter in. The practice of selling on organized exchanges (e.g., commodity exchanges, stock exchanges) or at auction also introduces special factors (**13, 14, 35, 42, 259**).

7. *Number of competitors, and domination of market by a company or by a competitor.*

This is of basic importance in appraising the likely effect of a price change on sales (**13, 14, 35, 42, 259, 271**).

8. *Ease or difficulty of entry of new competitors into same line of production or distribution.* In some industries, the great size of the initial investment needed, the existence of legal barriers, or the unavailability of patented processes may limit the possibilities of additional competition (**13, 14, 35, 42**).

9. *Extent of coordination of action as between companies.* Cooperation, parallel action, or collusion of sellers or buyers may characterize some market situations (**11, 12, 35, 38, 68, 83, 101, 217, 259, 271**).

10. *Separation of ownership and control.* The wide ownership of some companies or the inactivity of major ownership interests may result in most significant decisions being made by hired managers. The motives and other factors affecting their decisions may be quite different from those of owners (**23, 26, 62, 127a**).

11. *Extent of public regulation (or ownership and operation).* Where prices and/or wages are publicly regulated, where profits are limited or non-existent, where accounts are made public, managerial decisions may be quite different from those of "free" enterprises.

12. *Existence of economic coercion* (through patent ownership, debt, monopoly, etc.).

13. *Social controls other than legislation* (mores, local social pressures, etc.).

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THE MISUSE OF CHI-SQUARE—A REPLY TO LEWIS AND BURKE

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In the November, 1949, issue of the *Psychological Bulletin*,[1] Lewis and Burke discuss "The Use and Misuse of the Chi-square Test" in a long article in which they allege misuses in all but three out of 14 articles, and charge the Peters and Van Voorhis textbook with six misses out of seven tries. In our case every one of these six criticisms is invalid, although they could have made, but did not, a seventh criticism which would have been valid. If the same distribution of validities holds for the other writers (a possibility that I have not investigated), the situation may not be so bad as they make out. Lewis and Burke play up only some partial applications of chi-square at the expense of more fundamental and general considerations, thus frequently overshooting the mark in their generalizations. They make no mention of Karl Pearson's 1900 article in the London-Edinburgh-Dublin *Philosophical Magazine* (Vol. **50**, pages 157–175), in which chi-square was first announced; yet no one can expect to have a basic understanding of this statistic without a thorough reading of this article and of Pearson's follow-up articles in volumes **14**, **24** and **26** of *Biometrika*. We shall take up in turn some wrong allegations in the Lewis and Burke article.

1. *That chi-square applies fundamentally only to frequencies.* It is more general than that. The starting point for the derivation of the distribution function of chi and chi-square, which is always assumed for the "makings" of these statistics, is the normal distribution function for standard scores:

$$df = \frac{1}{\sqrt{2\pi}} e^{-z^2/2} dz \qquad [i]$$

By methods recently available, the distribution function for chi and for chi-square come rather easily from this normal distribution function. They are:

$$df(\chi) = \frac{1}{\frac{n-2}{2}\,!\,2^{(n-2)/2}} \chi^{n-1} e^{-z^2/2} d(\chi) \qquad [ii]$$

[1] LEWIS, DON, & BURKE, C. J. The use and misuse of the chi-square test. *Psychol. Bull.*, 1949, **46**, 433–489.

$$df(\chi^2) = \frac{1}{\frac{n-2}{2}!\,2^{n/2}} (\chi^2)^{(n-2)/2} e^{-z^2/2} d(\chi^2) \qquad [iii]$$

where n is the number of degrees of freedom.[2]

In this derivation no conditions whatever are imposed about the nature of z except that it shall be a deviation from a true value, that the z's shall be normally distributed about this central value in the population, and that the deviations of which they are composed be divided by the true variance. The deviations may be individual variates or frequencies or means or any other statistics, provided only that they are normally distributed in the population, are from a true central value, and are divided by the true variance. However, aside from the case of frequencies, we can not often know the true variance and (somewhat less rarely) the true central value; but sometimes we can.

2. *That the sum of the theoretical frequencies must always equal the sum of the observed frequencies, and that our example reproduced in Table 1 of their article violates this principle.* The generalized definition of chi-square is:

$$\chi^2 = \sum \frac{(\mu - x)^2}{\tilde{\sigma}^2} \qquad [iv]$$

where μ is the true central value and the tilde over the σ means that it is the true standard deviation. The true variance for the frequency in a cell is $N\tilde{p}\tilde{q}$, where $\tilde{p}$ is the true proportion belonging in the cell and $\tilde{q}$ equals $(1-\tilde{p})$. So the generalized formula in the case of frequencies would be:

$$\chi^2 = \sum \frac{(f_t - f_o)^2}{N\tilde{p}\tilde{q}}. \qquad [v]$$

This involves nothing whatever about what the frequencies shall sum to. In a setup such as ours of Table 1 in the Lewis and Burke article the f_o could be made to sum to equality with the f_t only by artificial doctoring, except in very rare cases. Yet, so long as the "cramping" features about to be discussed are not present, [v] is the basic formula to apply to get the correct chi-square.

But Pearson showed, in the original article referred to above, that *if* the condition is imposed upon a universe of samples that the total N shall always be the same, and the same for theoretical and observed frequencies so that the sum of the errors is zero, then a system of intercorrelations is introduced of which account must be taken in getting

[2] Lewis and Burke do not give quite correctly the distribution function for chi-square. In their terminology, the exponent of chi-square should be $(r-2)/2$ instead of $(r+2)/2$.

the values for chi-square. For under these restrictions, an excess of frequencies in one cell must necessarily be accompanied by a deficiency in others. He set up the system of intercorrelations for this condition along with the squared deviations, worked them through an elaborate and highly technical solution by determinants, and emerged with the now well-known formula:

$$\chi^2 = \sum \frac{(f_t - f_o)^2}{\bar{p}N} = \sum \frac{(f_t - f_o)^2}{f_t}. \qquad [vi]$$

This is the formula that *must* be used when the limitation is imposed that the frequencies in a set shall always sum to N so that intercorrelations are involved; but, just as unequivocally, it must *not* be used otherwise.[3]

3. *That the frequencies of "non-occurrence" must always be present.* There is by no means always such necessity; it depends upon the nature of one's problem. One of the studies to which violation of this is charged is that by Grant and Norris, in which these investigators fitted a logarithmic curve and tested the agreement of the theoretical frequencies distributed to categories under it with the same aggregate of frequencies distributed according to observation. They applied the chi-square test in the customary manner for such frequency distribution testing. In such a procedure it would be entirely meaningless to set up for each category the number *not* in the category and compute chi-square elements for this second set of deviations to add to the ones conventionally used.

In their footnote on p. 435, Lewis and Burke also charge us with violation of this principle. In our example, statistics were given for the number of Italians, Russians, Poles, and Others who had been naturalized out of totals for those nationals in a community. The p was inferred from the sample, and the distribution tested for possible nationality differences. The chi-square was computed from only the row of "successes" by formula [*vi*]. We made the customary assumption that the total for the row is to be constant from sample to sample, and our generalization was about a universe of samples of which that description would hold. For our assumption of a fixed marginal total for the row, our procedure was in exact conformity with the conditions for which Pearson developed formula [*vi*]: a fixed N (the total number of naturalized) in a series of samples, f_t equal to f_o so that the sum of the errors in each sample is zero, a shifting incidence of the fixed number of naturalizations to the different nationalities in successive samples with

[3] We made a slip in using the customary formula, [*vi*], in connection with our dice throwing example when we should have used formula [*v*]. If formula [*v*] is used, chi-square is 12 with the same number of degrees of freedom. If Lewis and Burke had pointed this out, it would have been a valid criticism—the only one I acknowledge as legitimate.

the inevitable intercorrelations that would result, deviations of the frequencies in each cell from a theoretical number, and a test whether the deviations in this sample exceed those which could fall within the credible limits of sampling fluctuation. The degrees of freedom for this universe of samples are 3 and the p is .0024.

To install in this table a row of "non-occurrences" (the non-naturalized) would be essentially to establish a series of ratios, one for each nationality group, apply formula [*vi*] to each with N being the number in that nationality group to get a chi-square for it, then sum across these two-cell columns for a composite chi-square for testing the homogeneity of these ratios. Karl Pearson says (*Biometrika*, Vol. **14**, p. 418, and Vol. **24**, p. 353) that a table thus set up is not a true contingency table to which the chi-square test is appropriate. At any rate, it automatically freezes the N_c's for the universe of samples, which is sometimes a natural and therefore desirable feature and sometimes is not. Instead of following blindly a rule regarding the nature of the universe about which one shall generalize, the researcher should make a choice of such universe according to what is most meaningful in his particular problem, then say what his universe is. In our naturalization problem, for example, we can state what the probability is that as great discrepancies in ratio as we found in our sample would arise merely by sampling fluctuation in a universe of samples of size N taken at random from the same or similar total communities; that $P(df=7)$ would be .000138. Or we could state what would be the probability of the obtained discrepancies in a sub-universe of samples with always exactly the same numbers of Poles, Russians, etc. and exactly the same aggregate number of naturalizations and non-naturalizations; that $P(df=3)$ is .000002. There is a whale of a difference, although here both, of course, are statistically highly significant refutations of the null hypothesis. In cases like this one the former generalization would seem to be much the more natural and meaningful. It requires, of course, that the values obtained from the sample be taken as acceptable estimates of the true values; but Pearson argued, very persistently and to this writer very convincingly, that these estimates are quite satisfactory and that the gain from using them is greater than the loss; and he supports his arguments with experimental evidence. An awkward and stilted generalization is too great a price to pay for excessive finicalness about true values. However, the fourfold-table (if used) is an exception, if one wishes outcomes that are comparable with those of differences between proportions or of tetrachoric correlations; for the standard error formulas of both of these statistics assume fixed marginal totals.

4. *That chi-squares may not be summed where the same set of individuals is used in the series, because the sets are not independent; and that this principle is violated in our dice throwing example of their Table* 1. That idea rests upon a misunderstanding of the nature of independence.

When you throw a set of dice, they fall according to some law actually ***within*** them, within the limits of sampling fluctuation involved in that law. You also know the credible limits in the distribution of samples according to some *external* law which you suspect may be the law operating within the sample. You apply a chi-square test to see whether the behavior of the sample lies within the credible limits according to the external law. If it does not, you reject the hypothesis that your external law may be the law operating in the sample. You have made this test for one cell. Then you make a second throw and in it the dice behave again according to the law within them, quite independently of their behavior during the first throw. And so on with successive throws. The reason for summing these is merely to increase the reliability of the test and thus get a more dependable generalization for *these* dice. To do anything else than use the same dice, or the same subjects, would rob your investigation of all meaning for testing that hypothesis. But if on a second throw you picked up only *some* of the dice while allowing the others to lie and be counted in the next throw as they lay in the former throw, the sets would not then be independent. So if you are intending to generalize about the bias of *these* dice, or *these* subjects in a guess-heads or a choice-of-values experiment, it is correct to sum successive throws of the same dice, or successive responses of the same subjects. But if your purpose is to generalize about a wider universe of dice or of subjects, then a new sample of that wider universe must be chosen on each successive trial. Then you can generalize about the wider universe from which the samples have been randomly drawn. Our generalization was specifically about *these* dice.

With dice this procedure could scarcely go awry. But, with human or animal subjects, if something happened between trials that might affect the next ones (as information about success), the chi-squares could not be meaningfully summed because we would have on the different trials not further samples from the same universe but each time a sample from a different universe. To study thus in a series of summed trials the bias of just *these* dice or *these* students may not be a very useful form of research, because the universe is too narrow to be, ordinarily, of much social importance. But it is sound statistically if one's purpose requires it.[4]

5. *That the view that the number of degrees of freedom appropriate in testing the fit of a normal curve depends upon the hypothesis to be tested is erroneously attributed to Karl Pearson.* It is not erroneously attributed

[4] The example in our dice throwing of Table 1 was not, of course, the report of a research. It was intended merely as a schematic example for illustrating the meaning of deviations from a true value, worked up in the conventional manner, excessively simplified for pedagogical purposes. The objection that it would be a poor research, and that the frequencies are too small, seems under the circumstances to be picayune.

to Pearson; it is correctly attributed to him, as a reading of *Biometrika*, Vol. **24**, pp. 359 and 361 will show.

6. *That certain writers, including us, recommend a chi-square test of goodness of fit of regression lines which is a rather poor one.* This should have been attributed to R. A. Fisher, not to either of the two writers to whom reference is made. We merely passed on Fisher's scheme, though unfortunately with our blessing. But in a footnote on the same page we referred the reader to "a better test of the goodness of fit of regression lines" in a section of our book where a test is developed in terms of the unbiassed correlation ratio, which is a statistic closely related to *F*.

Much of the difficulty with chi-square arises from a frenzy to extend its use into areas where it is not needed, because we have for them better statistics. All applications of chi-square with one degree of freedom should be cleared out with one sweep of the broom. For with one degree of freedom the distribution of chi is normal, so that nothing remains at that level that is distinctive for chi-square. Lewis and Burke say that for one *df* the square-root of chi-square (which is chi) is distributed as Student's t with N equal infinity. That is true because the distribution of Student's t with N equal to infinity *is* normal; but that fact does not follow from the stated distribution function of Student's t, because that contains no χ. But it does follow easily from our formula [*ii*]; by substituting 1 for n and remembering that χ^0 equals 1 and that $(-\frac{1}{2})!$ (which equals gamma $\frac{1}{2}$) equals $\sqrt{\pi}$, formula [*ii*] reduces to formula [*i*] (with, however, 2 in the numerator instead of 1, which does not affect the *shape* of the distribution and which compensates for the fact that chi's run only from zero to plus infinity instead of from minus infinity to plus infinity). If one applies in a four-fold table the well-known technique of a difference of proportions divided by the conventional standard error of a difference of proportions and goes to the normal curve for the interpretation of the probability, he will get the same result as by the chi-square technique to the hundredth decimal place—provided he is testing the same hypothesis and remembers that there belong in the standard error formula the "true" values of p and q. Even amateur statisticians know that you may not carry ratios from proportion to the normal curve and claim exactness for your probability in researches with such N's as we ever get in practice. But these same persons will go blithely and confidently ahead with chi-square, not knowing that none of the chi-square table entries hold strictly for N's short of infinity, and that this principle is violated just as badly in chi-square as it is in proportion.[5]

[5] There is much talk about what minimum number of frequencies is required in a cell before using it in a chi-square calculation: whether five or ten or twenty, or what. The number required for *exactness* in fulfilling the assumptions is infinite; for *any* number

Tetrachoric correlation, which can be applied to any four-fold table where the variates make a continuous distribution, also has a constructive meaning which shows the nature and the strength of the law that is present (while chi-square shows only whether *some* law is present without indicating either its nature or its strength). Tetrachoric r is, by the cosine-pi formula, as easily computed as chi-square; it assumes a normal distribution only over the table as a whole, not in the separate cells; and it has a good and reasonably simple test of reliability.

There are also available now more convenient tests than chi-square for testing the goodness of fit of the normal curve (because simpler), and for testing the goodness of fit of all regression lines. There remains little more for chi-square than the test of multiple contingency tables with greater complexity than four-fold. If research workers would not, in a mad rush to use chi-square, raise an unnecessary dust, they would not need to complain that they can not see.

This note has involved criticism of six allegations made by Lewis and Burke in their article. But, in spite of these particular criticisms and possibly some others that might have been made, their article contains many sound and useful ideas, and serves a good purpose. But it should be read with critical alertness, not taken offhand as gospel.

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short of infinity the determination is only rough, but good enough. In the binomial $(p+q)^N$, where $p+q$ equals 1, it is only as N (the frequency in the sample) approaches infinity that the distribution approaches the normality assumed for chi-square. When p (the proportion of the total N of the whole sample that belongs in the cell) and q are unequal, the distribution remains markedly skew far beyond the number usually mentioned as the minimum; but for any *definite* p it always approaches normality (around p) as the N of its exponent approaches infinity (and hence as the f in each cell approaches infinity). The damaging fact is not so much the intrinsic smallness of the f_t of the cell as it is the smallness of the p representing the proportion of the total number in the sample that belongs in the cell.

SOME COMMENTS ON "THE USE AND MISUSE OF THE CHI-SQUARE TEST"

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Lewis and Burke in their recent article in this journal[1] rightly call attention to the necessity for familiarity and understanding of the assumptions underlying the chi-square test. It is unfortunate, however, that some of the crucial points in this rather lengthy article are either incorrect or confusing. The purpose of this comment is to indicate some of these incorrect or confusing statements.

1. There is a typographical error in the statement of the equation of the distribution function of chi-square (p. 439, equation 8). The numerator of the exponent of χ^2 should be $(r-2)$ instead of $(r+2)$.

2. On p. 434f. Lewis and Burke discuss a number of errors which were made by Peters and Van Voorhis in illustrating a chi-square problem. In this discussion Lewis and Burke fail to present or indicate a possibly superior method for dealing with the problem. Such a method would involve the determination of the proportion of the fourteen throws which yield exactly 0, 1, 2, $\cdots$,12 aces. The proportions are the successive terms of the expansion of $14(q+p)^{12}$, where $p=1/6$ and $q=5/6$. The number of degrees of freedom would then be 11. (In the Peters and Van Voorhis example, however, the chi-square test is not applicable, as Lewis and Burke themselves point out, because the theoretical frequencies are too small.)

3. In connection with the previous example, Lewis and Burke raise another objection to the procedure followed by Peters and Van Voorhis: " . . . the observed frequencies lack independence. They lack independence because the same twelve dice were thrown each time" (p. 434). This is an irrelevant objection. The usual assumption is that the wear and tear on a set of twelve dice are unrelated to the probability of the specified event. Furthermore, if the purpose is to determine whether a particular set of dice is biased, the thing to do is to experiment with the given set of dice. Lewis and Burke imply that the set of dice should be changed with each successive throw.

4. On pp. 441–443 the authors present and discuss a presumably "incorrect" application of the additive property of chi-square. Actually the application of the additive property of chi-square to this example is correct.

Ninety-six students were each asked to guess the fall of a coin five times. The actual turn of the coin was never revealed. "The hypothesis under test is that the guesses of students *on five successive tosses* of a

[1] LEWIS, DON, & BURKE, C. J. The use and misuse of the chi-square test. *Psychol. Bull.* 1949, **46**, 433–489.

coin are purely chance occurrences, with the probability of a guess of heads (by any student on any toss) equal to the probability of a guess of tails" (p. 442).[2] In other words, the authors propose to study the response-tendencies of students in order to determine whether such response-tendencies represent the same type of occurrence as tossing a set of five unbiased coins 96 times. It is assumed that each set of coins is properly shuffled before each toss. On the basis of the hypothesis, and the assumed independence of each toss, it is possible to calculate the expected number of heads for each set of 96 tosses. Since the coin is assumed to be unbiased the expected number of heads is 48. On the basis of this theoretical picture it is possible to calculate the probability of obtaining either a set of observed frequencies, or a set of more divergent frequencies. This probability can be calculated for each of the five coins. The authors calculate the chi-square value for each coin and then determine the appropriate probability with one degree of freedom. (See Table 3, p. 442). The relevant question is whether the five values of chi-square can be added, as well as the degrees of freedom, in order to determine the over-all probability for the set of five chi-squares. The authors claim that this addition is incorrect because "between tosses, the five guesses of each student were undoubtedly interrelated" (p. 443). Actually, however, the addition is quite permissible. It is correct that the additive property of chi-square assumes that the individual chi-squares are independent. This assumption correctly characterizes the theoretical model against which the observed frequencies are compared. If the probability corresponding to the sum of the chi-squares is small (less than five per cent or less than one per cent) then we may infer either that an unusual event has occurred, or that the hypothesis underlying the theoretical model is incorrect, or that some condition underlying the chi-square test was violated.

Although the authors deny the applicability of the additive property of chi-square to the particular example, they do affirm the applicability of chi-square to the first toss of the coin because "on a single toss, the guess of each student was independent of the guess of the other students" (p. 443). Following the thinking of the authors, the chi-square text should not be even applicable to the first toss because the guess of one student is not necessarily independent of the guesses of the other students. There could be a general cultural bias in favor of heads on the first toss. In addition, in the same sense in which the second guess of a student may be related to his first, the first guess may be related to some psychological tendency of the student.

5. On pp. 478–480 the authors present an example in detail in order to clarify the meaning of "independence between measures." The discussion of this example is quite confusing.

Each of 240 students was asked to guess the fall of four successive

[2] Authors' italics.

tosses of a coin. The authors use the binomial formula in order to determine the probability distribution of the number of hits. It is assumed that the response of a subject is just as likely to be "head" as "tail." Since the probability of a hit is $\frac{1}{2}$ the theoretical number of 0, 1, 2, 3, 4 hits can be calculated. This distribution is then compared with the observed distribution of hits. The value of chi-square in this problem is equal to 43.351, with four degrees of freedom (see Table XXV, p. 479). Since the probability associated with this value of chi-square indicates the occurrence of a very unusual event, one would expect that the authors would reject the hypothesis. Since the original hypothesis was that the probability of a hit was equal to $\frac{1}{2}$, an alternative hypothesis would be one for which the probability of a hit is greater than $\frac{1}{2}$. The practical inference is that either the individuals responded with some knowledge of the actual fall of the coin or that the average response patterns of the subjects happened to coincide with the fixed pattern of the four coins (*H T T H*).

Rather than reject the hypothesis or question the associated conditions, the authors come to the remarkable conclusion that "The absence of independence and the consequent inability to obtain unequivocal theoretical (chance) frequencies made this application of the chi-square test a meaningless procedure" (p. 480). By the phrase "absence of independence" the authors mean that there is some psychological linkage between the responses of a subject to the four successive tosess of a coin. Thus it is that the authors reject the application of the binomial formula in this particular problem (p. 479). It should be noted, however, that the application of the binomial formula assumes independence in a statistical sense, viz., that the joint probability of two random events is equal to the product of the respective probabilities of the two events. The binomial formula can be correctly applied to the problem on hand because the probability of getting a hit is not affected by the psychological tendencies of the subject. Of course, since the pattern of the four coins was fixed, *H T T H*, it could happen that psychological tendencies of the subject may produce a disproportionate number of hits. To obviate this factor it is necessary to toss the four coins for each trial. The thing which is at fault, if there is any fault, in this application is not the binomial formula but the structure of the experiment.

6. In a criticism of a problem cited from the literature, Lewis and Burke fail to mention the significant point that the sum of the probabilities is greater than unity (viz., 1.7), whereas the sum of the probabilities must be equal to unity (Table XXVI, p. 481). Moreover, the authors' own alternative analysis of the same problem contains the same error, the sum of the probabilities being somewhat larger than 1.8 (Table XXVII, p. 482).

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ON "THE USE AND MISUSE OF THE CHI-SQUARE TEST"—THE CASE OF THE 2×2 CONTINGENCY TABLE

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In their recent article in this journal, Lewis and Burke (7) provide an excellent guide for the research worker in psychology in terms of what to do and what not to do in applying the χ^2 test of significance. It is, of course, obvious that there are conditions which will invalidate any test of significance. In the case of the χ^2 test, authorities tend to agree that one of these conditions is the presence of small theoretical frequencies. That Lewis and Burke are seriously concerned about what they believe to be violations of this principle by psychologists is indicated by the fact that warnings against the use of small theoretical frequencies appear through their paper. For example:

> The use of "small theoretical frequencies" is listed as one of the "principal sources of error" in applications of the χ^2 test by psychologists in articles published in the *Journal of Experimental Psychology* over a three-year period (7, pp. 433–434).
>
> "The commonest weakness in applications of the χ^2 test to contingency tables is the use of extremely small theoretical frequencies" (7, p. 460).
>
> If we have but a single *df*, as in the case of the 2×2 contingency table, "the use of theoretical frequencies of less than 10 should be strictly avoided" (7, p. 460).
>
> "Whenever small theoretical frequencies enter into calculations of χ^2, the experimenter has no sound basis either for accepting or rejecting a hypothesis except when the value (of χ^2) is quite extreme" (7, p. 462).
>
> Several applications of the χ^2 test by Lewis and Franklin (9) are judged not acceptable because "they involved extremely small theoretical frequencies" (7, p. 433).

In this paper we shall be concerned only with applications of the χ^2 test to the 2×2 contingency table. More specifically, we shall be concerned with what constitutes a small theoretical frequency in this application.

1. There is no quarrel with the idea that a large number of well-controlled observations is more satisfying than a small number of well-controlled observations. However, if only a limited number of well-controlled observations can be made, this may be more satisfying than a large number of poorly-controlled observations. At the same time, with other conditions remaining constant, a small number of observations will yield smaller theoretical frequencies than a large number of observations. Let us grant, then, that a large N is a good thing, if observations are well-controlled, and that we like to have it when possible. This, in turn, means that we like to avoid small theoretical frequencies. There is no disagreement on this point.

Disagreement does occur when we ask the pertinent question: How large must a theoretical frequency be before it is considered not small? Or, to turn the question around: How small may a theoretical frequency be before it is considered not large? The problem of adequately defining smallness, in the case of the χ^2 test, is, as Yule and Kendall (12, p. 422) have pointed out, not a simple one.

Cramér (1), according to Lewis and Burke, "firmly recommends a minimal value of 10" (7, p. 487), and Lewis and Burke are quite explicit in stating that in their opinion: "A value of 5 is believed to be too low" (7, p. 462). Yule and Kendall (12, p. 422) take a more moderate view. They regard 5 as a minimal value, but add that 10 is better. Hoel (5, p. 191) suggests that 5 is satisfactory if the number of cells or categories is equal to or greater than 5. But this is not the case with the 2x2 contingency table and, in this instance, Hoel suggests that it is better to have theoretical frequencies "somewhat greater than 5" (5, p. 191). Fisher (3, p. 87, 97), on the other hand, seems content with the rule of 5.

The position taken in this paper is that psychologists (and other research workers) are not necessarily "misusing" the χ^2 test when they apply it to the 2x2 contingency table when a theoretical cell frequency is as low as 5.

2. The exact probability for any set of frequencies in a 2x2 table can be obtained by direct methods of calculation as given by Fisher (3, pp. 100–102). At the same time, it is important to emphasize, as Fisher has, that the χ^2 test applied to the same data "can only be of approximate accuracy" (3, p. 97), and that its usefulness lies in "the comparative simplicity of the calculations" (3, p. 100).

The "approximate accuracy" of the χ^2 test can be judged by the extent to which the probability associated with the obtained value of χ^2 is in accord with that obtained by the direct method. Agreement between the two values of P is likely to be best when theoretical frequencies are not small. The reasons for this are well stated by Lewis and Burke (7, pp. 439–440). This does not mean, however, that χ^2 is not useful for its "approximate accuracy" in estimating the exact probabilities in 2x2 tables involving theoretical frequencies as small as 5. Nor is the application of the χ^2 test in this instance a "misuse" of χ^2.

Consistently rejecting a null hypothesis when P is .05 or less and accepting the hypothesis when P is greater than .05 is a practice followed by many research workers. It is suggested here that the research worker will not be led badly astray in evaluating the 2x2 contingency table with theoretical frequencies as small as 5 (1) if he consistently follows the P equal to or less than .05 standard (or some other standard which is acceptable); (2) if his attitude toward the null hypothesis is based upon the value of χ^2 corrected for continuity; and (3) if he calculates the exact probabilities for those cases where the probability value obtained by the χ^2 test is of borderline significance.

In practice, as Fisher (3, p. 83) states with his usual common sense attitude, our interest is not primarily in the exact value of P for any

given hypothesis, but rather in whether or not the hypothesis is open to suspicion.

3. Let us examine the two sets of data given by Lewis and Burke involving a 2x2 table with small theoretical frequencies. Table 1, given below, is their Table XVI, and is based upon the data of Lewis

TABLE 1

CLASSIFICATION OF SUBJECTS WORKING UNDER DIFFERENT CONDITIONS ON THE BASIS OF THE RATIO (*RI/RC*) OF THE NUMBER OF INTERRUPTED TASKS RECALLED TO THE NUMBER OF COMPLETED TASKS RECALLED*

RI/RC	*Individual Work Condition*	*Cooperative Work Condition*	*Total*
Greater than 0.67	6	12	18
Less than 0.67	6	2	8
Total	12	14	26

* This table corresponds to Table XVI in the Lewis and Burke article (7, p. 461). The data presented are based upon experiments by Lewis (8) and Lewis and Franklin (9).

(8) and Lewis and Franklin (9). With respect to this table, Lewis and Burke state that "all four of the theoretical frequencies . . . are too small to warrant an application of the χ^2 test" (7, p. 461).

Introducing Yates' correction for continuity in one of the standard computational formulas for χ^2, we have

$$\chi^2 = \frac{N\left(bc - ad - \frac{N}{2}\right)^2}{(a+b)(a+c)(b+d)(c+d)}, \qquad [1]$$

where the letters a, b, c, and d correspond to the cell frequencies and N represents the total number of observations. Calculating χ^2 for the data of Table 1, we obtain a value of 2.374. And since χ, the square root of χ^2 with 1 df, is distributed as a normal deviate z, we find from the table of the normal curve that when $z = 1.54$, P is .0618. The corresponding value of P for χ^2 will be (2) (.0618) = .1236.[1] What is the "approximate accuracy" of this value in terms of the probability obtained by direct methods?

Assuming the constancy of the marginal totals, as is assumed in calculating χ^2 also, the probability of any observed set of frequencies is given by the product of the factorials of the four marginal totals, divided by the product of the factorials of the grand total and the four

[1] The reason why we take $2P$ for the probability associated with χ^2 is discussed in Edwards (2) and Goulden (4).

cell entries (3, pp. 100–102). In terms of a formula[2]

$$P = \left(\frac{(a+b)!(a+c)!(b+d)!(c+d)!}{N!}\right)\left(\frac{1}{a!b!c!d!}\right). \qquad [2]$$

The desired probability, however, involves not only the arrangement of frequencies as given in Table 1, but all other possible arrangements (deviations) which are more extreme. Thus we need the probabilities for the following arrangements:

6	12	5	13	4	14
6	2	7	1	8	0

The probability desired will then be the sum of the probabilities for the three arrangements. Direct calculation shows this to be .0612, a value which may be compared with the probability of 0.618 for χ. We also have (2)(.0162) =.1224 with which we may compare the probability of .1236 for χ^2. Not only would no conclusions concerning significance be changed, if χ^2 is calculated with the correction for continuity, but the "approximate accuracy" of the χ^2 test seems quite good in this particular instance.

Table 2 reproduces Kuenne's (6) data as given by Lewis and Burke in their Table XVII and for which they are also concerned about the

TABLE 2

CLASSIFICATION OF SUBJECTS IN TWO AGE GROUPS ON THE BASIS OF THOSE SHOWING TRANSPOSITION AND NON-TRANSPOSITION*

Age Groups	*Transposition*	*Non-Transposition*	*Total*
3–4 years	3	15	18
5–6 years	20	6	26
Total	23	21	44

* This table corresponds to Table XVII in the Lewis and Burke article (7, p. 462). The data are based upon an experiment by Kuenne (6).

application of the χ^2 test because of small theoretical frequencies. From formula [1] we find χ^2 to be 13.1585, and χ to be equal to 3.627. From the table of the normal curve, in the manner previously described, we find that the value of P corresponding to χ is .0001433, and for χ^2 we have (2) (.0001433) = .0002866. Direct calculation, in the manner described earlier, shows that the corresponding values of P are .0000985 and (2) (.0000985) = .0001970, respectively. Again no conclusions concerning significance would be changed, regardless of whether the data

[2] A table of the logarithms of factorials such as may be found in Pearson's tables (10) or the *Mathematical Tables from Handbook of Chemistry and Physics* (13) facilitates the necessary calculations.

are evaluated by means of χ^2 or by the direct method. As to the "approximate accuracy" of the χ^2 test, it may be observed that the discrepancy between the two values of P for χ^2 and for the direct method is .0000896.

A few additional examples taken from Goulden (4) and Yates (11) illustrate the degree of "approximate accuracy" which χ^2 may give, even when theoretical frequencies are smaller than the value of 5. In these examples the probability obtained by the direct method has been given by the source from which the data are taken. χ^2 has in all cases been corrected for continuity. For simplicity, the value of P is given for χ and as obtained by the direct method. The value P for χ^2 and the corresponding value for the direct method may be obtained by doubling the reported values. Examples 1, 2, and 3 are real in that they are based upon actual data, and Example 4 is simply an exercise taken from Goulden.

Example 1—Hellman's data cited by Yates (11).

	Normal Teeth	*Malocclusion*	*Total*
Breast-fed	4	16	20
Bottle-fed	1	21	22
Total	5	37	42

$\chi = 1.068$: $P = .1427$
Direct method: $P = .1435$

Example 2—Grant's data cited by Goulden (4).

	Group Blood 0	*Blood Group Not 0*	*Total*
Fond du lac Indians	18	11	29
Chipewyan Indians	13	1	14
Total	31	12	43

$\chi = 1.75$: $P = .0401$
Direct method: $P = .0349$

Example 3—Mainland's data on the position of polar bodies in the ova of the ferret cited by Goulden (4).

	Similar	*Different*	*Total*
10μ apart	5	1	6
More than 10μ apart	1	6	7
Total	6	7	13

$\chi = 1.93$: $P = .0268$
Direct method: $P = .025$

Example 4—Exercise given in Goulden (4).

	Recovered	*Died*	*Total*
Animals inoculated	7	3	10
Animals not inoculated	3	9	12
Total	10	12	22

$\chi = 1.68$: $P = .0465$
Direct method: $P = .0456$

As will be apparent, each of the examples cited involves theoretical frequencies of less than 5, yet the degree of "approximate accuracy" of the χ^2 test seems satisfactory. We should not expect, however, that all 2x2 tables with theoretical frequencies as small as these will yield P's, when tested by means of the χ^2 test, which are as close to those obtained by the direct method.

4. It is to be emphasized that there is no magic involved in the rule of 5 as a minimal theoretical frequency—any more than there is in the rule of 10. It will be true, however, that if we calculate the value of P directly whenever the theoretical frequency is under 10, we shall be involved in roughly about twice as much labor as would be the case if we accept 5 as the minimal theoretical frequency.

The procedure suggested here is to apply the rule of 5 and to calculate χ^2, keeping in mind the correction for continuity. If the obtained value of χ^2, in such cases, is of borderline significance, then calculate the value of P by the direct method. If theoretical frequencies of less than 5 are involved, the desired probability may be found, not too inconveniently, by the direct method.

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FURTHER DISCUSSION OF THE USE AND MISUSE OF THE CHI-SQUARE TEST

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The articles by Peters (5), Pastore (4), and Edwards (2) objecting in one way or another to our paper on chi-square (3), indicate either that the paper was not carefully studied or that our exposition was less clear than we had supposed. Since Peters and Pastore raise several points in common, we are forced to conclude that the fault was ours to a considerable extent. A clarification of the points in question will be attempted.

We should perhaps say, parenthetically, that we are confident we could have written a clear, concise paper in mathematical language which would have left little or no room for disagreement between Peters, Pastore, and Edwards, on the one hand, and ourselves, on the other. Because the paper was aimed at the average user of statistics among psychologists, we rejected a mathematical type of exposition in favor of proceeding largely by example. We are disappointed but not surprised to discover that our treatment has been found obscure in a few places.

Reply to Peters

Before the writing of the paper was begun, we carefully surveyed the basic literature on chi-square, including several papers by Pearson, but could see no good reason for basing our discussion on the earliest proofs. Full credit is due Pearson for originating the chi-square test. His proofs are adequate for establishing the general validity of the statistic. However, advancements have been made in the mathematical methods applied to statistical problems; mathematicians have been able to improve upon many of the early proofs. Such recent treatises as Cramér's *Mathematical Methods of Statistics* (1) provide a more complete background for the χ^2 distribution function and for the tests associated with it, and they serve as a suitable point of departure for any discussion of χ^2. In making this statement, we intend no disparagement of Pearson. He falls easily among the four or five most eminent statisticians. It is a commonplace in science that later investigators, often persons of lesser ability, using the insights of a great innovator, frequently improve upon his work.

The specific points made by Peters will be discussed in the order in which he presents them.

1. *That we claim chi-square applies fundamentally only to frequencies.* An examination of our equations [1], [2], and [8] and of the comments in footnote 8, page 439, and on page 487 will show that we do no such thing. Our equation [8] is the exact equivalent of Peter's equation [iii], except for the error in the exponent of χ^2 which Peters and Pastore both mention. Several persons have written since November calling our attention to this error, and we discovered it ourselves soon after it appeared in cold ineradicable print. We will be greatly indebted to anyone who can provide a foolproof method of avoiding errors of this kind.

What we do claim is that the formula

$$\chi^2 = \sum \frac{(F_o - F_t)^2}{F_t} \qquad [10]^1$$

can be legitimately applied only to raw frequency data. Applications involving frequencies are the ones of greatest interest to most psychologists, so it seemed appropriate to emphasize these applications and to enumerate certain fundamental assumptions and restrictions through a discussion of the binomial and multinomial distribution functions. We are well aware of the fact (stated in our footnote 10) that the chi-square distribution function may be derived from the joint normal distribution function in n variables, and aware of the additional fact (stated several times in the paper) that the chi-square statistic plays an important role in the study of variances.

2 *and* 3. *That the sum of the theoretical frequencies need not equal the sum of the observed frequencies, and that the frequencies of non-occurrence need not always enter into the calculations.* These two points can best be discussed together, as they are intimately related.

Cramér (1, p. 426 ff.) rigorously proves an underlying theorem on chi-square and indicates how the proof of a generalization of the theorem would be handled. The generalized theorem is broad enough to include as special cases all applications of chi-square which are based on formula [10]. It encompasses the tests of fixed probabilities, tests of goodness of fit, and tests of independence, and shows clearly the conditions which must prevail for other tests of the same general kind. It includes the case covered by Pearson as well as the new derivation requested by Peters in his third footnote, and establishes the fact that *the number of degrees of freedom is the same in both cases.* In Cramér's proof, it is necessary to assume that the frequency associated with every possible outcome of the experiment is used and that the sum of the theoretical frequencies is made equal to the sum of the observed frequencies. It should be emphasized that these restricting assumptions hold only for applications of chi-square based on formula [10].

[1] The formula carries this number in the original paper.

Peters states that he and Van Voorhis erred in one of their examples (6, Table XXXV) by using his formula[vi] instead of [v]. This is simply an alternative way of wording one of our objections. We show in our paper (3, pp. 437–438) that using his formula [v] is exactly equivalent to using our formula [10] and taking account of frequencies of non-occurrence after the sums of the observed and theoretical frequencies have been equated.[2] What Peters does, in effect, is to accept our criticism but insist that it be stated in another way.

Peters maintains that the application of chi-square which he and Van Voorhis made in relation to numbers of naturalized citizens among several nationality groups is a correct one. Since a careful examination of this application is instructive, we give in Table I, Part A, observed frequencies (of occurrence and non-occurrence of naturalization) adapted from Peters and Van Voorhis' Table XXXVI. In Part B of the table are the theoretical frequencies as calculated by Peters and Van Voorhis on the assumption of homogeneity of nationality groups with respect to the proportion naturalized. Note that the marginal totals in the two

TABLE I

OBSERVED AND THEORETICAL FREQUENCIES FROM TABLE XXXVI IN PETERS AND VAN VOORHIS (6, p. 414)

A. OBSERVED FREQUENCIES

	Italians	*Russians*	*Polish*	*Others*	*Totals*
Number Naturalized	161	82	20	32	295
Remainder	205	34	32	22	293
Totals	366	116	52	54	588

B. THEORETICAL FREQUENCIES

	Italians	*Russians*	*Polish*	*Others*	*Totals*
Number Naturalized	183	58	26	28	295
Remainder	183	58	26	26	293
Totals	366	116	52	54	588

parts of the table are identical. The theorem given by Cramér provides unimpeachable proof that the quantity obtained by using formula [10] and summing over all eight categories will have a distribution approximating the χ^2 distribution with 3 *df* provided that the hypothesis of homogeneity is true. In this example, as can be readily verified, the value of χ^2 based on the eight categories is 29.72. Peters and Van Voorhis used formula [10] also, but excluded the "remainder" categories and

[2] In this particular case, by using [v] Peters obtains a correct value of 12, as he reports in his second footnote (5, p. 332). Using equation [10], we obtained the correct value of 12 and so stated in our fourth footnote (3, p. 435). This agreement is not a coincidence.

summed over only the top four categories, to obtain a value of 14.52. Since their computations included only half of the categories, they would obtain for any similar sample a value considerably smaller than we would obtain, regardless of the truth or falsity of the hypothesis. The value computed from all eight categories is known to have, approximately, the χ^2 distribution with 3 *df*, when the hypothesis is true. It follows, of course, that the quantity they computed cannot have the distribution they attribute to it because it is systematically too small.[3] Thus, there is no logic for interpreting their statistic by means of the χ^2 distribution with 3 *df*. The fact that the decision reached in this particular case would be the same for both computational methods is completely beside the point.

Peters seems to imply that there are no differences between an application of this kind and the tests of goodness of fit for ordinary distribution functions. Where, we ask, in testing the goodness of fit of a normal curve, do categories of "success" and "failure" ever exist over the same class interval? The difference between the applications is really quite profound.

4. *That no difficulties arise in applying chi-square in situations where the presence of individual differences may bring about a lack of independence.* Here Peters misunderstands what we mean by *independence*, a misunderstanding that often arises because the term is used with at least four distinct meanings in connection with chi-square tests. We have in mind the well-known distinction between the Bernoulli-De Moivre theorems on the large sample binomial distribution (based on constant probabilities) and the Poisson generalization of these theorems (including variable and linked probabilities). A somewhat extreme, but simple, example will clarify our objection to the dice-throwing illustration used by Peters and Van Voorhis.

Suppose that we have 20 coins which are of three kinds. With two of them, the probability of obtaining either a head or a tail is $\frac{1}{2}$; but nine of the coins have heads on both sides while the remaining nine have tails on both sides. If we toss the 20 coins together we will observe either 9, 10 or 11 heads, the remainder being tails. From such a toss, the value of χ^2 computed with formula [10] with theoretical frequencies of 10 must be either 0.0 or 0.2, with a probability of $\frac{1}{2}$ associated with each. If we were to toss the coins five times and combine the results as Peters and Van Voorhis did in their dice-throwing illustration, we would obtain for the computed quantity the following exact distribution:

χ^2	0.0	0.2	0.4	0.6	0.8	1.0
P	1/32	5/32	10/32	10/32	5/32	1/32

If a quantity having this distribution were interpreted in accordance

[3] On the same grounds, we reaffirm our statement that Grant and Norris erred in omitting frequencies of non-occurrence.

with the χ^2 distribution, the hypothesis that the coins are unbiased would never be rejected since, with 5 *df*, a χ^2 value larger than 1.0 has a probability of occurrence of over 0.95. And yet, we know that the coins are biased.

Too much emphasis should not be placed on the incorrectness of adding χ^2 values in a situation of this kind. The results would not be much better if the hundred turns (20 coins each tossed 5 times) were treated as a unit. The use of formula [10] with the pooled results, the theoretical frequencies now being 50, would yield a quantity having the following exact distribution:

χ^2	0.0	0.04	0.16	0.36	0.64	1.0
P	126/512	210/512	120/512	45/512	10/512	1/512

Again, the hypothesis that the coins are unbiased would never be rejected through the use of the χ^2 distribution because, with 1 *df*, the probability of obtaining a value of χ^2 larger than 1.0 by chance is over 0.3.

In order to make a meaningful study of the coins, we would be obliged to specify (if we could) some event which had a uniform and not a varying probability of occurrence. With a population of coins of the three specified types, we might test the hypothesis that the probability of selecting a coin at random, tossing it, and observing a head is $\frac{1}{2}$. To do this, we would need to select different coins for each repetition of the experiment (a procedure which apparently surprises Pastore).

We would not object quite so strongly to the dice-throwing illustration of Peters and Van Voorhis if there were not a simple modification in procedure which eliminates the difficulty. If each die were tossed a sufficient number of times to permit the computation of separate values of χ^2 to represent the "behavior" of the separate dice and the χ^2 values were then summed over the set of dice, the deviations due to individual differences would appear. In this design, there could be no compensating effects from one die to another. The same general procedure could be used for generalizing to a larger collection of coins.

The objection may be raised that our coin example is too bizarre to be taken seriously. (Who ever heard of a coin with heads or tails on both sides?) Our answer is that, in any situation where individual differences in the underlying probabilities exist, the same effect is present although usually not so conspicuously. We are inclined to believe that individual differences among coins and dice may actually contribute to their ways of turning. We insist that with subjects in psychological experiments, individual differences cannot be ignored.

Peters finds us picayune in our discussion of one of his examples, and his attitude is not entirely unwarranted. However, for better or worse, we are forced to judge his book partly by the uses our students and colleagues make of it. Some psychologists are prone to take any example in an authoritative text as a model of experimental design and

to copy it in their own work. The text by Peters and Van Voorhis is widely used, and justifiably so; in many respects, most notably in its exposition of mathematical backgrounds, it is an admirable book. This wide use imposes certain obligations on the authors. We suspect that they may be partly responsible for experimental designs which they would certainly not sanction. This is admittedly a poor way to judge books, but in the field of statistics we have no other recourse. Because of the unexpected uses to which examples may be put, we believe that correctness should never be sacrificed to pedagogical simplicity.

5. *That we erred in not attributing to Karl Pearson the notion that a quantity computed according to fixed rules can have two different distributions depending on the choice of the experimenter.* We readily concede that Pearson discusses this point in almost the same way that Peters and Van Voorhis do. And yet, we find a certain ambivalence in his position. At times, he writes as if he regarded his recommendations as approximations which are acceptable only on empirical grounds. It comes down to a matter of interpretation, and we doubt that anything can be proved one way or the other. We retract the statement to which Peters objects as being too firmly worded and admit that his interpretation of Pearson's views may be the correct one. However, the retraction does not alter the fact that the number of degrees of freedom for χ^2 in situations of the type discussed by Pearson and Peters is unaffected by the way the investigator phrases the hypothesis under test.

6. *That the χ^2 test of linearity of regression is adequate in some circumstances, but can be replaced by a better test.* This is precisely the view taken in our paper. We might have attributed the χ^2 linearity test to Fisher, as Peter says we should have done, but it was not our aim to designate the originator of every technique we mentioned. For our purposes, the best points of departure were the formulas and recommendations in the texts and reference works most widely used by psychologists.

We share many of the views expressed by Peters in his closing paragraphs but are less sanguine than he concerning the likelihood of finding suitable replacements for χ^2.

Reply to Pastore

Several of Pastore's criticisms have been answered either directly or indirectly in our reply to Peters. Those which remain will now be discussed, with the numbering kept the same as his.

2. He contends that we fail to offer a better method for handling the dice-throwing problem of Peters and Van Voorhis. We actually present a correct method (**3**, pp. 446–447) indicating that separate values of χ^2 should be obtained for the individual dice and that these

values could then be summed. This procedure is without flaws. The method proposed by Pastore is also a correct one and would avoid the difficulties posed by the possibility of individual differences, but a large number of throws (much larger than 14) would be required. Also, because there are 13 categories, there would be 12 *df* instead of 11, as stated by Pastore—provided, of course, that enough throws were made to yield theoretical frequencies of acceptable size in all categories.[4] With the technique we suggest, the particular dice (if any) which were biased could be spotted; with Pastore's technique, this would not be possible.

4. Pastore maintains that values of χ^2 can be combined even when they are known to be interdependent, provided that there is independence in the underlying model. It is made quite clear in our paper (3, p. 442 ff.) that we are discussing a case in which linkages between guesses on separate trials are empirically known to be present. Frankly, we are rather surprised at being asked to defend the thesis that a test should not be based on a demonstrably false assumption, and wonder why anyone would wish to use a model which is already known to be inappropriate. However, we shall present an artificial example of a rather extreme type to clarify the major issue.

Suppose that we run a coin-guessing experiment of the kind described in our original paper, but someone instructs the subjects to alternate their guesses throughout the experiment. We happen to select a group of 100 subjects such that, on the first guess, 55 responses are heads and 45 are tails. The resulting value of χ^2 is 1. On the second guess, the responses are 45 heads and 55 tails, and the value of χ^2 is again 1. After 20 guesses, we would have a composite χ^2 value of 20, with 20 *df*. Since a value larger than this would have a probability greater than 0.4 of occurring by chance, we would retain the hypothesis that each guess was mediated entirely by chance factors. But in this situation, the only random event is the first guess. We wonder if Pastore would wish to consider this set of guesses as a purely chance affair throughout.

If the experiment were repeated under the same conditions, 43 guesses of heads and 57 guesses of tails might be obtained on the first trial. The resulting value of χ^2 would be 1.89. Larger values than this have a probability roughly equal to 0.15 associated with them. The difference between 1.89 and 1.00 is not large and little importance would be attached to it. After 20 guesses, however, we would have a composite value of 37.8, significant at the 1% level. Thus, under these conditions,

[4] The probability of 12 aces on a single throw would be about .000000000459, so it would take around 20 billion throws to give a theoretical frequency of 10 in that category.

first-trial differences which are unimportant can lead, after summation, to rather extreme differences in interpretation.

Whenever interdependencies exist, effects of the type just illustrated will be present. Pastore's extension of our argument to the first guess is clearly illegitimate. Any deviation of the frequencies from the theoretical values to be tested can be unambiguously interpreted and no difficulties arise. Also, if a different sample of subjects is used on each guess in obtaining a composite value of χ^2, there are no interdependencies to complicate the result.

5. It is asserted that, in one of our examples illustrating indeterminate theoretical frequencies, the test which we reject is a correct one. Since a detailed discussion of this point turns upon interdependencies of the kind already treated in detail and would merely be a repetition of an argument previously given, we shall confine ourselves to a few general remarks. (If one considers what would happen with a group of subjects who were systematically alternating their guesses, the difficulties become quite clear.)

Pastore may be justified in saying that the obtained value of $\chi^2(=43.351$, with 4 *df*) is large enough to warrant a rejection of the hypothesis without further question. But here again, he shows a willingness to use a model that is known to be inapplicable. What can be gained from such a procedure? If the hypothesis is rejected, nothing is gained because it was already known to be false. If it is not rejected, it still cannot be retained in view of its known falseness.

Pastore writes as if a statistic can be correct for an experiment even though the structure of the experiment is faulty. As far as we are concerned, the experimental design and the statistical treatment constitute a unit and must be treated as such. Incidentally, we should like to learn Pastore's technique for testing the hypothesis that a probability is greater than $\frac{1}{2}$.

6. Pastore objects because we include (**3**, pp. 481–482) two sets of probabilities which sum to more than unity. Considered in their proper setting, they are obviously conditional probabilities. The sum of the corrected probabilities is unity in each case, since the sum of the observed frequencies is equal to the sum of the theoretical frequencies. Perhaps this point can be made clear by an example which is a close parallel to the case under consideration.

Suppose that someone offers us a gift which is hidden under one of three upturned cups. The gift is to be ours if we can guess the cup under which it lies. The probability is $\frac{1}{3}$ that we will guess right. Our first guess is wrong, but we are invited to try again. The cups and gift remain as they were. The probability of a correct guess is now $\frac{1}{2}$. Following our second incorrect guess, a third one is proffered. Our memory is sufficiently good—under ordinary circumstances—to make the third probability 1.0. The sum of this set of probabilities is 1.83.

Reply to Edwards

Edwards' paper (2) seems less a criticism of ours than an attempted advance based on a new collection of evidence; and we doubt that there are any serious disagreements between him and us. Our paper, although strongly recommending a minimal value of 10 for theoretical frequencies, allows for smaller values in the 2×2 table and proposes the use of Fisher's exact treatment whenever they occur. Edwards argues for frequencies as low as five and for the use of χ^2 as a sort of screening device and the use of the exact treatment as a check. The difference in viewpoint is not very great.

We may have overstated our case. At least Edwards' examples are quite convincing as far as they go, and his recommendations seem reasonable.[5] Nevertheless, we are inclined to take a position of "wait-and-see." In many cases, as shown by Edwards' examples, chi-square tests based on small theoretical frequencies are not far off, and there appears to be the possibility of proving that such tests will never be far off. Until such proof is forthcoming, however, the possibility must be accepted that combinations of numbers in fourfold tables may exist for which the divergences could not be neglected. In this connection, we will be quite happy to see final proof that we are wrong.

As a final word, we wish to express the sincere hope that everything we have written on chi-square will (to borrow a phrase from Peters) "be read with critical alertness, not taken offhand as gospel."

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[5] With reference to his reworking of the frequency data obtained by Lewis, Lewis and Franklin, and Kuenne, we disavow in our paper any criticism of the experimental conclusions reached by these investigators.

BOOK REVIEWS

DASHIELL, J. F. *Fundamentals of general psychology.* (3rd Ed.) Boston: Houghton Mifflin, 1949. Pp. x+690. $4.00.

It is gratifying to have a new edition of Professor Dashiell's well known and respected text, *Fundamentals of General Psychology*, in a new format and written in a style which should hold student interest without loss of scientific soundness. The general outlook of the book has changed little and remains essentially eclectic with a leaning toward the language of stimulus-response psychology. However, this edition reflects the increased interest in dynamic problems which is evident among academic psychologists. A few examples will illustrate this point. In 1937 Freud's familiar defense mechanisms were discussed but without reference to Freud. In the new edition Freud is mentioned in a number of places, though somewhat reluctantly and with a warning to the student. The Freudian concept of personality structure is presented and the notion of libido is mentioned briefly in the section on sex drive. In addition, Dashiell now makes use of the ego as an organizing factor in motivation and finds the concepts of identification and levels of aspiration to be useful in this connection.

The student is presented with a large amount of original experimental material, and in general one cannot quarrel with the accuracy of the reporting. It is clear and to the point. We must however, question at least one interpretation. Dashiell, in common with a great many writers, talks about determining the percentage of a person's achievement which is due to heredity and to environment, and suggests that the results indicate the contribution to be "fifty-fifty." There is of course *no* method available for determining the percentage of *a person's* achievement or development which is due to heredity or to environment. The fact is we can know only that a certain percentage of *variation* between individuals in a group is due to the *differences* in these factors.

It is always important to ask how a text handles information about the tools and techniques of psychology, for the intelligent layman needs to develop critical skills in these areas if he is to be protected against half-truth. Dashiell discusses various kinds of observation and measurement in almost every chapter. However, despite the fact that considerable space is devoted to relatively complicated statistical concepts, such as correlation and critical ratios, one misses the explanation of what psychological measurements mean in logical terms. There is, for example, no reference to the limitations of relative measures, to the absence of absolute zero or of equal units. It is the reviewer's experience that these concepts are more easily understood and more usefully employed by the elementary student than is the arithmetic of correlation. Also

missing is a description of the problem of the inferred variable. Dashiell discusses attitudes and sets and motives and other concepts of this sort which the psychologist must use in his science. He does not make clear to the student that such notions are different in a very important way from the more direct observations of stimulus and response, which are regarded as fundamental. The student of general psychology or of any science should become familiar with the fact that inferences of this kind are among the most important activities of the scientist, whether he be a physicist or a psychologist.

Textbooks vary in their degree of organization around systematic principles. The Dashiell text takes a middle course in this respect. The principles used are eclectic and not always definite. This is true, for example, of the attitude toward learning. Learning is "organismic adjustment." The author describes various learning processes, admits that the law of effect holds if it is looked at broadly enough, accepts contiguity, sees that learning may be facilitated by observation, and says that some day, "all partial, single-viewpoint theories will become successfully incorporated into one complete and completely satisfactory system of learning principles." He does not attempt to indicate what this system is likely to be, but it must be admitted that there is a real question whether such a commitment can be made at the elementary level without unwarranted dogmatism.

A final word should be said about the treatment of perception, for this illustrates better than most topics the fact that Dashiell retains the spirit of a stimulus-response approach in spite of his greater use of what is generally called dynamic material. The chapters on sensory functions, set, attention, and perceiving are widely separated. This results in what seems to the reviewer to be a lack of integration of the problems of sensitivity, judgment, perception and motivation. But for any author the words of his sentences must proceed serially and he cannot discuss everything simultaneously. This inevitably makes difficult the description of fluid and shifting and interrelated psychological processes which rarely fit the limitations of sentence structure.

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O'KELLY, LAWRENCE I. *Introduction to psychopathology.* New York: Prentice-Hall, 1949. Pp. xxi+736. $4.50.

O'Kelly's justification for the above title can be gleaned from his statement that "An examination of current texts in 'abnormal psychology' shows that the selection of subject matter has changed over the years, and that such texts are, in fact, psychopathologies at the present time and are not the collection of curiosities that include such 'normal' abnormalities as sleep and hypnosis. The phrase 'abnormal psychology' is as awkward and inappropriate to our modern knowledge of maladap-

tation as the phrase 'abnormal anatomy' or 'abnormal physiology' would be as a title for texts in tissue pathology." In this book the author has made an able and comprehensive start in demonstrating the fruitfulness of viewing pathological phenomena as instances of broad psychobiological principles. The student reading this book is not likely to come away with the idea that different principles are necessary to explain different pathological conditions.

This text is divided into three parts. In the first part (Introduction) the major emphasis is on the definition of basic concepts. The reader is not plunged into a discussion of pathological phenomena but rather is given some broad principles which are necessary for understanding any aspect of behavior. In the second part (The Problems of Disordered Behavior) there are better and more extended discussions of anxiety and psychosomatics than will be found in most other texts. This part also has an adequate coverage of the psychoses and psychoneuroses. The third part (The Causes of Disordered Behavior) contains discussions of heredity, mental deficiency, organic factors, social factors, theories of psychopathology, and treatment. Throughout the book the author's training and work in comparative psychology are reflected in his lucid and comprehensive handling of the physiological aspects of the subject matter.

This reviewer would recommend this book for undergraduate courses with very few reservations. There are several shortcomings which should be kept in mind when this text is used in graduate courses. The discussion on theories of psychopathology is meagre; problems in treatment and prophylaxis are also given too little attention; test data continue to be viewed as the sole criterion of mental deficiency; and cross-cultural data are not given the discussion they deserve. It should be said that these shortcomings hold for all other texts which this reviewer has read. The virtues of this book far outweigh its shortcomings. On the assumption that in class the instructor does more than tell students what they already have read in their assignments, this book can be heartily recommended for graduate courses.

SEYMOUR B. SARASON.

Yale University.

SALTER, ANDREW. *Conditioned reflex therapy—The direct approach to the reconstruction of personality.* New York: Creative Age Press, 1949. Pp. x+359. $3.75.

No psychologically informed person will accept this book, but neither will he be able to dismiss it lightly.

Here are some of the things that make the book vivid and arresting:

1. The author can write! His ability to turn a neat phrase and to express his meaning in terse, emphatic prose will be the despair of many an academician.

He knows how long to make a chapter and how to organize a book so as to sustain interest and be pedagogically effective.

2. The author is well read. Each chapter has a technical bibliography, and the book is seasoned with apposite literary allusions.

3. Whatever criticisms are leveled against the book, no one can fairly accuse the author of being clinically naive. He is manifestly experienced, hard-bitten. He often condenses into one or two sentences clinical inductions and insights to which more prolix writers would devote a paragraph or a page.

4. The author is right in maintaining that psychotherapy is a matter of *learning*. He is undaunted by medical pretensions, and unblushingly makes therapy a *psychological* enterprise.

Some of the patent weaknesses of the book are:

1. As the title—*Conditioned Reflex Therapy*—suggests, the author takes Pavlov as his principal authority. For him (the author), a neurotic is simply a person who in childhood has acquired the wrong ("inhibitory") conditioned reflexes. In therapy (by Salter) he acquires the right ("excitatory") ones. "The sickness is always inhibition, and the medicine is always excitation" (p. 300). Why old, inappropriate "conditioned reflexes" do not undergo spontaneous extinction (as Pavlovian theory would suggest) is a question which the author does not consider.

2. Explicitly, "learning" is equated to "conditioning." Yet the author repeatedly acknowledges a form of learning (trial-and-error, problem-solving) which is determined by motivation and reward. As an avowed reflexologist, he must deny the significance of drives, but he is not able consistently to maintain such a position.

3. Although severely critical of Freudian psychoanalysis, the author, in making impulse-inhibition and moral over-restraint the cause of neurosis, outfreuds Freud.

4. Although it is not entirely fair to characterize "conditioned reflex therapy" as symptom-treatment, there is much to justify such a characterization. Symptoms are not seen as solutions (habits) which the patient has developed as means of dealing with anxiety, but as "inhibitory conditioned reflexes." By means of "auto-hypnosis" (shades of Coué) which is based on (unreproducible) experimental findings reported by Huggins, the patient is given "exercises" which are supposed to replace the inhibitory reflexes by excitatory ones.

5. Sounder but less stressed parts of Mr. Salter's therapeutic procedure include: (a) encouraging the patient to engage in "feeling-talk"—which is reminiscent of James' admonition never to have an emotion without acting upon it; (b) emphasis upon changing self-attitudes by altering one's relations with other significant persons (cf. Sullivan); and (c) advice on breaking "vicious circles"—but the author here fails to face some of the implications of his own argument.

Conditioned Reflex Therapy is a defiant, impudent book! But who dares admit to complacency in respect either to theory or practice in the field of psychotherapy today? The author's approach is directive to a degree which will startle analysts only a little less than it will

client-centered therapists: "I then become stern. . . . I am the authority. I will decide" (p. 69). "You'll do just as I say. Do you understand?" (p. 169). "I'm running the show" (p. 181). But unless one dismisses as fraudulent the case histories and the changes in Bernreuter scores which the author reports, one must draw the inference that Mr. Salter is achieving results which are neither much better nor much worse than those of more conventional therapists.

Here, again, we face that pertinacious question: What *is* psychotherapy—its processes and principles? By the art of simplicity combined with bristling self-assuredness, the author makes out a case which the unwary will mistake for an answer.

O. H. MOWRER.

University of Illinois.

DERI, SUSAN K. *The Szondi Test.* New York: Grune & Stratton, 1949. $5.00.

The publication of Susan Deri's long awaited book on the Szondi method will, in the opinion of this reviewer, be to the Szondi what Beck's *Manual* was to the Rorschach in 1937. Both books were written by ardent disciples of a master; both bear the stamp of their author's original extension of a creative idea; and both were produced in response to the intense interest of a few at a stage of apparent readiness for sharing with the many.

Since Deri began her seminars in 1945, research under her leadership has grown in an atmosphere of mingled fascination and skepticism. American psychologists have found it impossible to take seriously Szondi's theory of choice based on affinity between the genes of a subject and the genes of a patient whose photograph is selected as "liked" or "disliked." The test has nevertheless had strong appeal, not only because of its ease and simplicity of administration and recording, but because the experience of many clinicians suggests that clinically valid interpretations of choice profiles can be derived even without fully acceptable explanation of the assumptions developed from this theory.

In her book, Deri loyally states Szondi's views, including the typology which appears in his own untranslated book. Then, with a tact which is unusual in professional writing, she sets his theory aside and proceeds to the description of her own rationale of interpretation. To this task she brings her experience as a practicing analyst, and her training under Lewin. The determinants of choice are discussed not in terms of genetic characteristics of pictured patients falling in eight nosological categories, but rather in terms of stereotypes which the pictures represent to subjects who select or reject them in terms of their own needs. Choice, according to Deri, is the result of tension or satiation in eight need-tension systems, grouped into four vectors, which provide "an octagonal gauge for the understanding of personality dynamics." Al-

though the need systems are reconciled both with Freudian constructs and with topological conceptualization of personality structure, the vectorial approach offers a new set of dimensions within which to conceive the dynamic processes of sexuality and aggression, cycles of control and discharge, ego structure and function, and object relationship.

To the extent that the clinical validity of the test proves verifiable, the Szondi test, as Deri describes it, promises several unique contributions. Through serial administration, it is possible to study test-retest change, thus capitalizing on a form of unreliability which in more cumbersome methods is an obstacle. Second, the interpretation of "loaded" and "open" factors, in terms of discharge and discharge readiness, allows the prediction of symptoms and overt behavior patterns in relation to stable, "root" factors in which may be sought the deeper, inner springs of action.

Although Deri's book does not in itself clear the mystery of why the test appears to "work," it offers a clear formulation of assumptions which should lend themselves to experiment. It is to be hoped that clinicians will not use the book as a manual of signs for the deceptively easy identification of syndromes and traits, but will utilize it as a source book of hypotheses for systematic exploration.

HELEN D. SARGENT.

Topeka, Kansas.

KORNER, ANNELIESE FRIEDSAM. *Some aspects of hostility in young children.* New York: Grune & Stratton, 1949. Pp. ix+194. $3.50.

This is a report of a study of hostile behavior in 20 nursery school children (11 boys and 9 girls) between the ages of 4 and 6 years. Its main value lies in the author's use of several different methods of obtaining the data, and her careful comparison and evaluation of behaviors and ratings of behaviors obtained under different conditions and by different methods.

All of the 4 to 6 year old children in one nursery school who were living at home with both parents were used in the study. They were predominantly Roman Catholic, of below average socio-economic status. In the first experimental situation each child was shown furniture and doll families representing his own family constellation. This material was used in telling a series of ten stories. The stories represented situations involving conflicts of the type which are prevalent in families with small children. Each story conflict was left unresolved, with the child requested to complete it, both in words and actions with the toys. In a later play session these and additional toys, as well as clay and crayons, were made available for the child to play with as he chose. Interviews with parents included questions about the children's actual behavior in situations like those described in the stories. The children were also rated by their nursery school teachers on their hostile be-

havior in school, and by the experimenter from her observations during the experimental play periods.

No correlation was found between the children's displays of hostility observed or rated in the different situations. The children could be classed, roughly, into four groups: those who were consistently hostile, those who consistently showed little hostility, those who were hostile in real life but mild in the play situations, and those who enacted very hostile play behavior but were mild in real life situations. Between or within these groups there was no clear tendency for differences in the adequacy of their emotional adjustment, for acceptance or rejection by their parents, or the parents' method or severity of punishment.

However, analysis of all the case material on a given child proved revealing of the dynamics of that child's behavior. There was evidence that those children who were rejected were profoundly affected, although their reactions varied.

The study is well done and is in general excellent. In only one instance does the author appear to make generalizations not warranted by her material. She assumes (p. 141) that these children have an unusually high number of symptoms of emotional maladjustment, and attributes this high incidence to their age. However, she gives no supporting evidence for either assumption. She cites no comparative data on incidence for other children, other ages, or children of different cultural or economic backgrounds. This particular point is a minor one, however, and does not affect the main conclusion of her study. A practical application of this conclusion is a warning to clinicians that a particular kind of behavior, such as hostility, should be interpreted with caution, and only in the light of other relevant information.

NANCY BAYLEY.

University of California, Berkeley.

BROUWER, PAUL J. *Student personnel services in general education.* Washington, D. C.: American Council on Education, 1949. Pp. xix+317. $3.50.

This volume is a report of some experiences in developing personnel programs in twenty-two colleges associated together in a cooperative study in general education. The point of view is stated on the book cover as: "all those who influence the educational experiences of students are personnel workers." On p. xi of the preface, a further identifying statement is the following: "analyzing ways to identify and satisfy the needs of students through the coordinated use of fact-finding devices of personnel services." A good deal of attention is given to a contrast between two types of counseling: (1) prescriptive emphases, identified with intellectual understanding by the counselor and communicated to the client; and (2) permissive emphases derived from the non-directive point of view and in which the atmosphere of the counseling interview

facilitates growth of the individual client. The latter point of view has been adopted by the author.

Included in the volume are chapters on the counseling process, the counseling program, extracurricular life, living arrangements, pre- and post-college personnel services, specialized personnel services, personnel services in the classroom, and administration of personnel services. Two personnel or counseling instruments are described, a self inventory of personal-social relations and an inventory of counseling relations. Chapter 14 contains an outline of a personnel philosophy of education which, to this reviewer, is largely the expression of an individualization movement in education, though, to be sure, this present statement includes mental hygiene and other emotional growth aspects of instructions. Part III is entitled, "The Principles of Personnel Services" and includes a number of sections. Sister Annette writes on psychological principles; Professor Mahoney discusses physiological principles; a chapter on philosophical principles is contributed by Professor Cannom; and a chapter on the sociological principles of personnel work presumably prepared by the author, Mr. Brouwer. These four interesting chapters make more explicit some assumptions regarding the nature of human nature pertinent to personnel work. These assumptions are identified in this volume: the unity of the individual; individual differences; and individual motivations as goals. In the chapter on sociological principles, socialization is described in terms of interiorizing the social environment from childhood to adulthood. The campus is spoken of in general terms as sub-cultures. Sister Annette defines adaptive counseling, ". . . . which aims to help the student to help himself. It is characterized by an adaptation of procedures to meet the particular needs of the student as they are revealed in the counseling interview." This is presented as a formulation mediating between a prescriptive and permissive emphasis in counseling theory.

This book is an interesting contribution to the meager literature of explicit emphases, points of view, and consequent experiences in the development of institutional programs of personnel services to students.

E. G. WILLIAMSON.

University of Minnesota.

VERNON, P. E., & PARRY, J. B. *Personnel selection in the British Forces.* London: Univ. of London Press, 1949. Pp. 324. 20 Shillings net.

The authors have linked up the developments of the last war in selection and guidance in the British Forces with the broader aspects of vocational and educational psychology. Some 80 pages (Part I) are concerned with the organization of selection procedures of the Navy, Army, Women's Corps and Air Force. Then the scientific contributions of this work are included in general discussion of familiar topics of personnel psychology. This forms the major portion of the book (Part II).

The book is written primarily for the Britisher, to show the industrialist and educationalist the usefulness achieved by psychology during the war years. It is limited in scope pretty much to British sources and it was not intended by the authors to survey American war-time psychological work.

The reader of this book will secure a general picture of work accomplished in personnel selection in the various British Forces. He will not find inclusive statements of war-time research projects. But their results are extensively quoted in connection with various topics in vocational psychology. The main RAF findings on selection are summarized in Ch. XVI. A useful reference will be found in Appendix II which lists by title and author, "The Main Psychological Tests Used in the Forces," giving the number of items, time of administration and reliability of tests.

The authors maintain the point of view that the "clinical" as contrasted with the "psychometric" approach yields a truer account of the candidate. There is an extensive discussion of the use of the interview (Ch. IX) and it is stated that "The interview rather than tests must remain the prime instrument of vocational classification" (p. 164).

Various "problems" indicated in the text will strike a chord of sympathy in American war-time researchers as in matters of military administration, "Without belittling in any way the fine work by all types and grades of staff, it must be admitted that the organization did not always function smoothly. Psychologists suffered considerable frustration. They were commonly of lower rank. . . . Policies they advocated as scientifically sound were often rejected, and the methods they devised were often misapplied and misinterpreted . . . " (p. 42); or in technical matters, "Some training courses were largely inappropriate; training devices and visual aids were employed without proof of their value; old-fashioned examination methods were used which not only probably failed the wrong men, but also provided thoroughly unsatisfactory criteria against which to gauge the validity of selection procedures" (p. 98).

The authors include an excellent summary of conclusions based on personnel selection in the Forces which they regard as having vocational and educational applications in peace-time. No new psychological methods or techniques of selection are described, but added factual material is supplied for their understanding.

The book is packed with facts from the work of selection in the Armed Forces of Great Britain and it will probably be the only available general source for this material. An extensive index has been prepared for reference work. By intent, the book does not deal with the work of psychologists on problems of training, the design and layout of equipment and the study of morale.

DOUGLAS H. FRYER.

New York University.

MURSELL, JAMES L. *Psychological testing.* (2nd Ed.) New York: Longmans, Green, 1949. Pp. xvi+488. $4.00.

A large part of this book (198 pages) is devoted to descriptions and critical comments on some 100 well-known tests of intelligence, aptitude, personality, interest, attitude, and character. This central core of the book is preceded by discussions of the general characteristics of mental tests and certain basic considerations affecting their use, such as validity and reliability, as well as by a chapter on the concept of general intelligence. It is followed by a treatment of mental growth and its relationship to heredity and environment, socio-economic factors, family relationships, schooling, and race. Constancy of the IQ, the distribution of mental traits, and the psychological significance of test scores are also discussed. The book ends with a chapter on the evolution and improvement of mental testing.

The author writes well and has read a good deal of the literature in the field of measurement; the text is liberally sprinkled with footnote references to such sources. It is probable that students will find the book readable and informative.

In the opinion of the present reviewer, however, the book is not scholarly in any sense of the term. It is easy enough to point out specific errors of fact and interpretation in it, but it is time consuming to demonstrate how fundamental misconceptions and misunderstandings of psychological testing have led to the subtle distortions and misrepresentations that are woven inextricably into the content. It reads as though an intelligent layman with a flair for writing but with no background in psychology or statistics had surveyed the literature on testing, taken a look at a hundred of the most commonly used tests, and then dashed off a book.

To illustrate the sorts of errors that characterize the text, let us consider the section on reliability in chapter 2. This section starts off "'If unchanging subjects are measured twice with a perfectly reliable instrument by a perfectly reliable agent, the correlation between the two sets of scores is 1.00' (Walker, p. 365). This statement briefly summarizes what is meant by reliability of measurement." The quotation from Walker is true, but it does not at all summarize what is meant by reliability of measurement. The classic definition of reliability by Spearman refers to the correlation of two separate but hypothetically equivalent measures. Later in the chapter (at the bottom of page 47), the writer contradicts an essential element in the formulation of reliability in terms of analysis-of-variance techniques when he writes, "Interdependent items, i.e., those which present the same problem in different forms tend to lower reliability." On the contrary, as item intercorrelation is increased by testing the same point in several ways, test reliability tends to increase.

On page 54, the author computes a standard error of measurement,

finds it to be 9.36, and then interprets it as follows: "This tells us that there is a 68% chance that a person making any given score on the first testing will score within a range of +9.36 to −9.36 of that score on the second testing. That is, if a person makes a score of 170 on one testing, there is a 68% chance that his score on another testing (which is often called his 'true score,' i.e., his score on *any other* testing) will fall between 161 and 179." These statements will not meet with the approval of educational statisticians.

If anyone supposes that the interpretation of test scores for practical purposes can safely be made by the author in spite of his apparent misunderstanding of the concepts of reliability, he should examine the statements at the bottom of p. 110 and the top of p. 111 concerning the interpretations of IQ's derived from the revised Stanford-Binet Scales. The author states, "Thus general reliability coefficients cannot be worked out for this scale, or for any such scales." The truth is that a general reliability coefficient for Stanford-Binet IQ's could easily be computed, but standard errors of measurement at several IQ levels are much more valuable for the informed test user than the general reliability coefficient.

In the reviewer's opinion, the faults of this book greatly outweigh any merits that it may possess. It does not seem to come up to the standard of the author's previous writings or to accomplish its stated purpose; in the reviewer's judgment, it should never have been published.

FREDERICK B. DAVIS.

Hunter College.

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ERRATUM

In listing the title of Muriel Potter's monograph in the January 1950 issue of THIS JOURNAL, The word "reception" was erroneously used for "perception." The correct citation is as follows:

POTTER, MURIEL CATHERINE. *Perception of symbol orientation and early reading success.* Teach. Coll. Contr. Educ. No. 939. New York: Bureau of Publications, Teach. Coll., Columbia Univ., 1949. Pp. viii+69. $2.10.